

# SCIENCE

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# Letters

## Anthropological Theories

Sidney Morgenbesser's article, "Role and status of anthropological theories" [*Science* 128, 285 (1958)], must give all anthropologists pause for reflection. He has made accusations, most of them just, and smashed icons, most of them already cracked. But in doing so in the pages of *Science* he has written not for anthropologists and philosophers but rather for a more general audience of scientists. To me, at least, the article seems a little misleading for such an audience, since the author did not mention the chief problem of anthropology. Anthropology's difficulties and its achievements can be somewhat better evaluated if that problem is squarely set forth.

Anthropologists gather most of their data from people many of whose ideas and activities are so foreign to our own as to merit the epithet "exotic." It would be convenient, if it were possible, to examine their modes of talking and acting, and classify them into some sort of pre-existing categories about which we could theorize. Anthropologists, in fact, did that for many years; some still do. But it must be remembered that "natives," thinking and acting "exotically," do not act at random. They have ideas and linguistic terms for organizing and communicating their activities. And once an anthropologist knows a culture well enough to communicate in their own terms with the people he is studying, he realizes that to reduce their modes of understanding to a set of pre-ordained categories may be to falsify them out of reason.

Here, then, is the basis of the anthropologist's problem: not only do ideas and acts exist; they are organized some way or another by the people who communicate about and act them. Yet, that organization is seldom—one could probably say never—adequate to allow generalization or broad comparison among cultures. Therefore, something beyond this folk organization of the facts is needed, namely, an analytical organization or a "scientific" theory.

Whereas a scientific theory in the physical or life sciences has to cope only with the facts and their theoretical organization, the behavioral sciences and particularly anthropology must deal with facts, with folk organization of facts, and only then with theoretical organizations of both. I do not mean that people are not sometimes wrong in their folk organization of facts, but only that if they are, that in itself may be the key fact.

Here, then, is the basic problem. The anthropologist always discovers some

sort of folk organization in his data. But this folk organization is not in itself adequate for his purposes, so he must create an additional organization—an analytical organization—in order to compare the facts and the folk theories.

Now, both the folk and the analytical viewpoints may be perfectly sound, but they must not be confused with one another. The folk organization has as its purpose action in daily life. Overt or covert, it is adequate to the extent that it is successful from the point of view of the actors. The analytical organization, on the other hand, has analysis and theorizing as its purpose. It should be entirely overt, and it is as adequate as the theory to which it leads. The direst trap for the anthropologist is to confuse folk organization with analytical organization. The most heinous fault that a work of anthropology can have is assignment of ideas from an analytical system to people who act merely in terms of a folk organization.

To take an example: a book on the law of an African tribe, say, based on sensitive and thorough field research, can be irrevocably spoiled if the author assumes a theoretical framework such as that established by Roman law, canon law, or common law and assigns these categories to the ideas and acts of Africans without first discovering and explaining the framework of "folk theory," so to speak, on which the Africans themselves organize the facts. To take another example: in the past, anthropologists have noted quite correctly that a certain ritual increases community solidarity (whatever definition they may have given for that), then spoiled it all by saying that the natives performed the ritual for the purpose of achieving solidarity, which of course they did not.

It is mistakes of this sort that Morgenbesser quite rightly holds in disrepute when he says that many anthropologists "fill their pages with references to vague entities, such as roles, institutions, and social structures, that ostensibly influence and mold behavior." Roles, institutions, and social structures are part of analytical systems. They cannot possibly affect behavior, which is organized in terms of folk systems. Only after a people has been educated to it can analytical systems sometimes play the role of folk systems. What Morgenbesser should have scored us for—we sometimes deserve it—is inability to distinguish data from analysis.

Morgenbesser's article glides over this whole problem in its last section, where he dismisses the theorists of "systems of belief." He attacks the term "system" when he ought to have attacked the word "belief." Anthropologists use the word "system" somewhat more loosely than Morgenbesser suggests. They mean by it

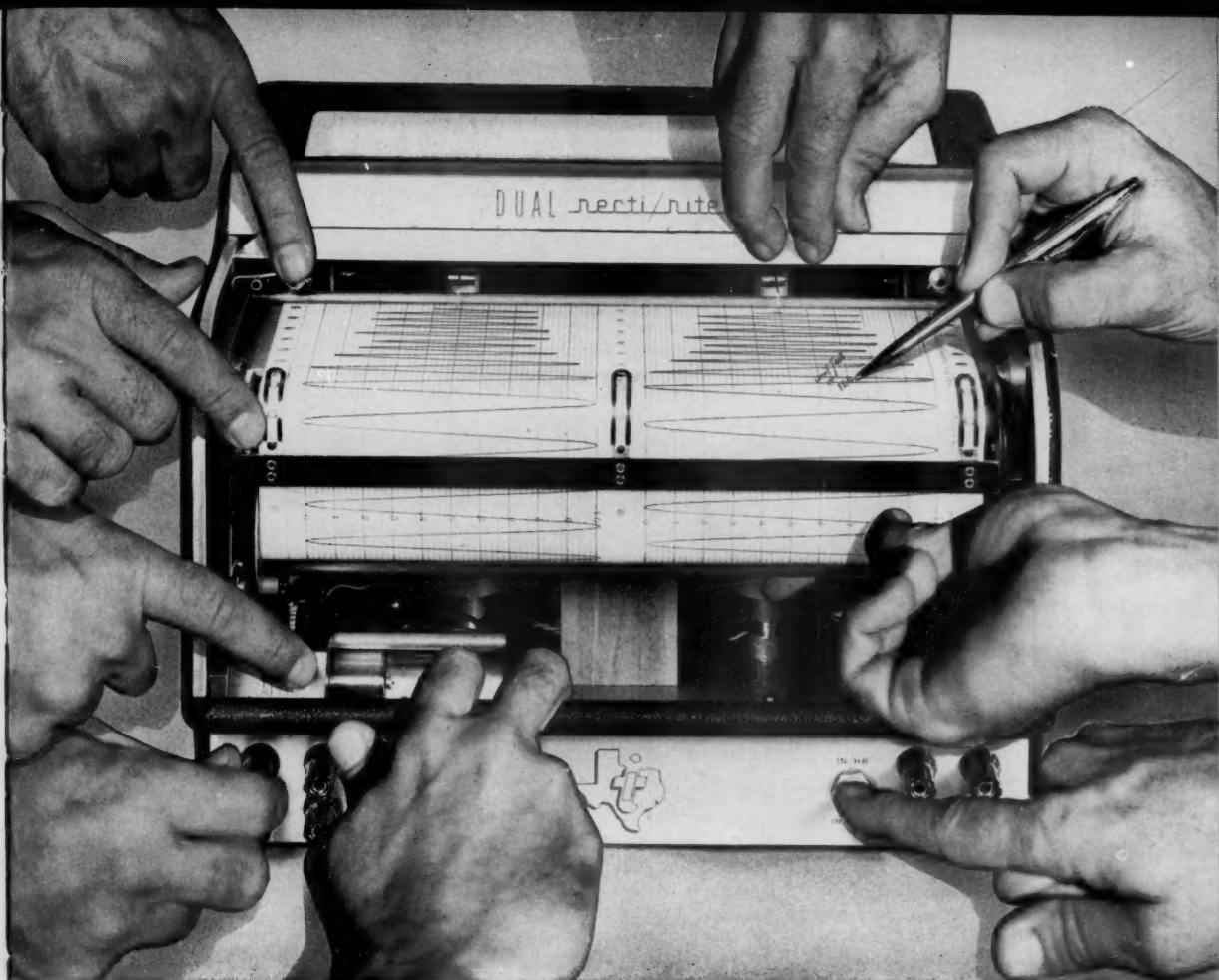
something that they might better call "consistency." But "belief" is more difficult. It is an interesting fact that many non-Indo-European languages have no term which even resembles our concept of "belief." What should be studied here, instead of beliefs, is "concepts" marked by "words." Words and concepts are systematized, in some degree at least, by the nature of language. And it is these words, and their combination into concepts, which are raw material for the anthropologist—not "beliefs," which are vague, undefinable, and subject to gross and nonregular distortion in communication. The folk organization of social and cultural activity, in other words, is in terms of concepts and the word tags by means of which people identify them.

As a matter of empirical experience, every culture has certain key words in its language. Almost without exception it is not possible for a person of that culture to say what they are. Those key words may be relatively few, as among some African groups or some American Indian tribes. They may be, on the other hand, so numerous that their very complexity makes it seem as if there were no keystones at all, or that this method of proceeding is not applicable in such an instance. This restatement of Ruth Benedict would seem to answer Morgenbesser's criticisms.

If an anthropologist learns the language of the people he studies—learns it really well—he should be able, in the process of translation, to pick out these terms: they are merely the terms which allow him to get his ethnography sensibly into as few folk categories as possible. I shall take examples from my own field work among the Tiv of central Nigeria. One such key word for Tiv ethnography is *tiev*, which means farm or garden. To describe the farms, the emotional attitudes to them, and work habits associated with them, is to set *tiev* into its native context. Another such word is *jir*, which means court, court case, moot. Tiv spend a great deal of time settling disputes in *jir*, and all of their ideas of justice and ethics among human beings eventually impinge on this activity. Another such term is *tsav*, a substance which grows on the hearts of "witches" and which forms the keystone of magical and religious ideas. Another is *kasoa*, which refers to what we would call a "market," but is much broader in concept than our term.

There are some half a dozen other such key words. If I write about these words, explaining them and their context fully, I can organize most of my field notes, and can communicate the major part of what I know about these people. A look into the work of any careful ethnographer will furnish just such a list of key concepts.





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I do not imply that every anthropologist, given the same situation, would pick out the same key words, but I do think that the overlap would be very great. We should also remember that a few years may make a great deal of difference in the culture of a people and in its key points.

Now, in many cases, key concepts are not, by any stretch of the imagination, directly comparable from one culture to another. In many other cases, of course, they are. The next problem, in any case, is in the communication of the full meaning of these concepts to an English-speaking audience—the English ideas have become “exotic.”

This particular form of translation is the anthropologist's chief problem. He must explain alien concepts in English without assimilating them to Western concepts, which would destroy them. The task can be achieved in only one way: erection of an “analytical system”—a “theory,” if anyone prefers the term—by means of which the Anglo-American or Western notions can be compared with the exotic ones in such a way that neither of them loses its individualizing traits—its “genius,” to use the 18th-century word.

It should be noted that I have now claimed that anthropologists are interested in ideas. And so I believe to be the case. They have got involved in sociology from time to time, and they have left it with such concepts as “role” and “culture,” on both of which sociology is today more dependent than is anthropology. They sometimes get involved with psychologists and with learning theory. As Morgenbesser has shown, they

sometimes handle it badly, but they have firmly established the notion of cultural relativity in psychology. Some anthropologists try to overformalize what they are doing and they end up with the “systems of beliefs” which Morgenbesser rightly denigrates.

But, in spite of these forays, the heart of anthropology remains its concern with the ideas of people and (when we include archeology and museology) with the tangible results of these ideas, or the tools and materials by means of which they are given expression—what we sum up as “culture.”

Anthropology resembles history in its processes of gathering data. We are interested in description of events, and in the generalizations and explanations made by the actors in those events. An anthropologist can acknowledge no finer master than Marc Bloch, the great French historian, in dealing with this aspect of his problem. Bloch's *Apologie pour l'histoire* sets the anthropologist's problem as ably as it sets the historian's problem.

However, when it comes to theory formation, anthropology resembles science. The rudiments of anthropological theorizing can be set forth in a diagram (Fig. 1).

The ethnographic, or “historical” aspects of our study allow us to arrive at box N via box M. Box N, then, is to be understood on the same level as box B, the folk explanations current in our own society and, on a different level but performing a comparable function so far as the anthropologist is concerned, the theories of the social sciences of our own society. Insofar as any idea in either box B or box N explains adequately everything in boxes A, B, M, and N, it is adequate anthropological theory and can be put into box X. If it does not, then anthropological theory must be a new and original statement of sensible relationships among these four boxes. The most common anthropological errors are (i) explaining box M in terms of box B, omitting box N altogether—that is, confusion of B2 and X; (ii) explaining box N by box B without first determining that the contents of box N can in fact be derived from box M.

Anthropological theory can, eventually, make predictions on the basis of manipulating the theory in box X. What it can never do is to make these predictions in terms of specific events unless it discovers a way to “feed back” the material of boxes M and N which were removed in process of arriving at the propositions in box X. Even then, accurate prognostication is extremely difficult, for the same processes may take place in many superficial, cultural forms and with many different event structures.

The difficulty in the past—and it is

one that is reflected in Morgenbesser's article—is that critics of anthropology have tried to say that it must be either history or a science. Actually, of course, it has elements of both and is perhaps not fully either. Anthropology is, like history or science, an “attitude.” That attitude consists in developing a stereoscopic view of human activities and human ideas by means of concepts that have been discovered in the processes of translating key organizing ideas from one cultural idiom into the language of another culture, usually our own, thus making these ideas in some degree available to all. Our theories, like those of the physical scientist, or indeed like the interpretations of historians, may go by the board. But our attitude, like the scientific attitude of which it is a mode, is here to stay.

PAUL BOHANNAN

Department of Economics and  
Sociology, Princeton University

Twenty years ago I was in substantial agreement with what Morgenbesser now says, and indeed published at that time a critique of the various brands of “functionalism” (1). I thought and said that much of the anthropological theory of that time was either trivial or trite, or noncogent. Indeed, it is perhaps still true that anthropologists have a singular knack of combining these three characteristics into a single utterance. But Morgenbesser criticizes the “functionalists” as though there were no other trends and hints of new ways of thinking in the anthropology of today. The persistence of the old I cannot deny. My purpose is to indicate that there is something new and that what is new is not trivial or trite and does not lack cogency.

I will first state what I mean by the nontrivial and in this connection I will be ambitious. It seems to me that a science has dignity and value insofar as it contributes to the next more abstract corpus of insights which lie behind it. The value of anthropology, in my estimation, lies in its contribution to general biological theory, to ethics, and to epistemology. These contributions are, however, very difficult to assess, and it is easier to judge whether, by unidentified contributions to the climate of more abstract thought, anthropology is actually making contributions at a more concrete level, to those other sciences which draw upon the same abstract background.

Can we assert that anthropological theory and experience would enable any neighboring specialist to ask more searching questions in his special field? Would a biologist modify his approach to the evolution of the Ammonites or to the bacterial theory of diseases because of the influence of anthropological

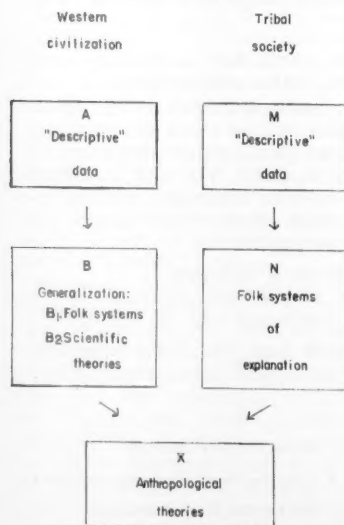
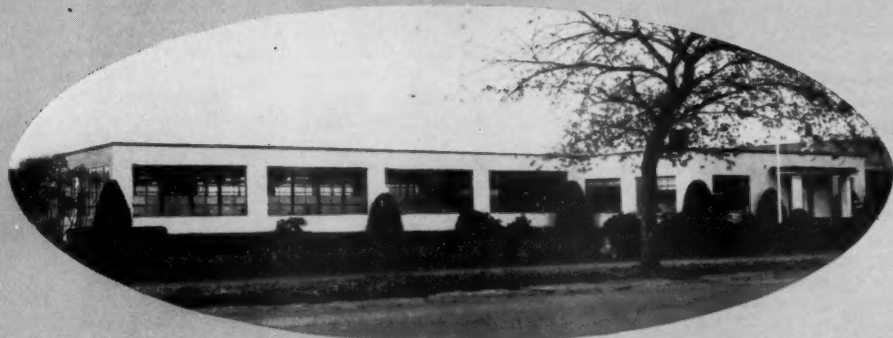


Fig. 1. Rudiments of anthropological theorizing.

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ing? Has anthropology contributed anything to the understanding of the paradoxical relationship between crime and mental illness? Has it contributed anything to the philosophical argument between those who regard reality as existing only in the eye of the beholder and those who feel that reality has independent existence?

I am willing to see the discipline stand or fall according to the answers to questions such as these. If to one such question the answer be affirmative, then we as anthropologists can be proud.

Before answering such questions, let me try to clear away some confusions which are present in Morgenbesser's attack. It is meaningless to suggest that psychology might dominate anthropology or vice versa. The delimitation of the disciplines psychology, sociology, economics, history, and so on, is in general now seen to have been a tragic error insofar as it hindered cooperation between them. Of course, in any wide field of inquiry there must always be specialists and there will always be a vocabulary for defining the special areas in which particular scientists elect to work. But within the field of behavioral science it is becoming evident that these various specialties have deep need of each other, and cannot go far without each other's aid. Now, curiously, the anthropologist has, especially since the days of Boas in America and Haddon in England, been the Pooh-Bah of the social sciences. Because the anthropologist has traditionally worked singlehandedly with the vast intricacies of the culture which he must study, he has had to combine—often in an amateurish fashion—the functions of all the different specialties. Anthropology cannot conceivably dominate any one of these because the specialties have their own order of expertise in their respective fields; and conversely, no one of the special fields can dominate anthropology because this subject draws upon the insights of all the others.

Indeed, it is fair to say that anthropology has contributed a very great deal to the recognition of interdependence among the specialties and to the development of what is called interdisciplinary research—a theoretical contribution of some importance.

However, this peculiar relationship between anthropology and the more special fields within behavioral science makes it a little difficult to answer some of the questions which I posed above. An incident in the evolution of learning theory provides a good example and one with which I am personally familiar. The psychological experimenters had concentrated their attention upon a particular order of learning. They had investigated the conditions under which a given organism will alter its response to a given

signal, and anthropologists had made heroic efforts to apply these findings to the analysis of native culture (2). It became evident, however, that such a limited theory of learning was inadequate for the anthropological discussion of social values. The particular order of learning which the psychologists had been able to study was *not* the order of learning with which anthropology (and psychiatry) were concerned. We wanted to know, not the answer to the question "Under what circumstances does a Balinese learn to dance?", but the answer to the next more complex question, "If the Balinese learn dancing under such and such circumstances, what conclusions can we draw from this about the character of the Balinese and their ways of interacting? Will they reduplicate in other contexts the formal patterns in which they have previously learned something?"

These considerations led to the formulation of a theory of "deutero-learning" (3), or learning to learn, for which there was already some experimental evidence within psychology (4), though the vast importance of differentiating the orders of learning had not at that time been recognized. More recently the matter has passed back into the hands of the psychologists who have provided the experimental demonstrations (5) which the anthropologist was unequipped to do.

It is appropriate to stress the fact that this shift in learning theory is in fact away from attention to the "psychology" of the learning subject, and toward a study of the *patterns of interaction* between subject and experimenter. The contexts used in earlier experimentation on learning—instrumental avoidance, instrumental reward, rote, Pavlovian conditioning, and so forth—now became elements in a vocabulary for talking about themes and patterns of interchange between organisms. And this shift was made possible precisely because anthropology is a nonspecialist subject.

Now let us look at the possible contributions of this sort of thinking to the questions which I posed above. The evolution of the Ammonites falls into place as a stochastic process to be thought about with the same sort of intellectual tools that are appropriate in problems of learning theory, and the biologist in this and similar fields will obviously do well to wonder whether there may be hierarchic orders of evolutionary change corresponding to the orders of learning mentioned above. He will also do well to wonder about the changes which he sees in his fossils, viewing these as possibly one end of an interactive process. At each step of the orthogenic sequence, did the environment or the surrounding organisms modify their characteristics so as to neutralize the advantage which the Ammonites had got from that forward

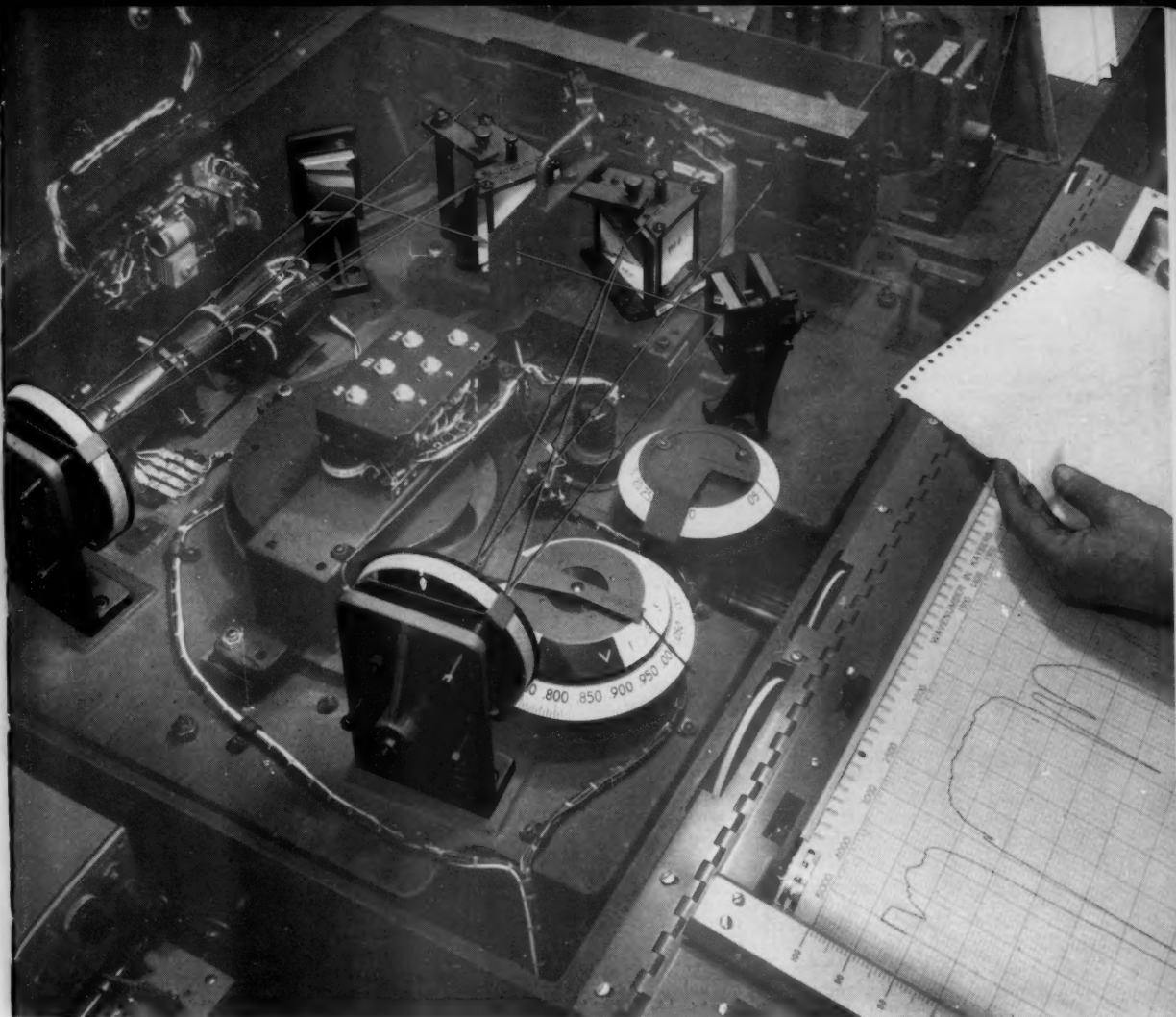
step? And did the environment thereby become such as to favor a further step in the same direction?

The question about bacterial theory points up the fact that anthropology is one of the disciplines contributing to a conceptual field of biology which we might call eco-genetics. Here the phenomena of evolutionary change, of learning, and of ecology can all be seen as closely interrelated. To any worker in this field it is no surprise that antibiotics in the long run tend to strengthen the defenses of the parasite while weakening those of the host.

The problems of reality and subjectivity are much more nebulous, but still it is possible that modern anthropological thinking is relevant even in this region. We, as nonspecialists in the whole realm of human behavior, are very much concerned with the subdivisions of this realm. What is "giving"? What is "trade"? What is "play"? We do not dismiss a piece of behavior as "mere" ritual; we ask what sort of communication is ritual. This brings us face to face with the theories of nominalism, realism, subjectivism, and so forth, not as abstract creations of the philosopher, but as categories or frames which determine and describe or classify behavior. To us, as anthropologists, the question for which men burned during the Reformation, namely, whether the bread and the wine are metaphoric or sacramental, is a meaningful question about people, and our contribution to abstract problems of this sort will arise from our study of human natural history. We shall say, not that there is one correct epistemology defining or answering these questions, but that epistemology is a body of statements about the ways in which human beings perceive and understand, and that these ways are themselves determined by—and determine—the interactive processes of learning.

However, in none of these answers can we claim that the thinking which anthropology contributes is in any sense the parthenogenetic offspring of anthropological theory. Our theories are themselves products of complex interactive processes, resultants of our interaction with other scientists, with philosophers, and with the multicultural world in which we live. The development and trend of anthropological thinking would certainly not be what they are without such sources as cybernetics, the theory of games, the ecological theories of Allee and others, and the thinking of such men as Russell and Whitehead.

And last, we owe a peculiar debt to the theorists of the previous generation, whom I once criticized and who now irritate Morgenbesser. The errors of Benedict, Radcliffe-Brown, and Malinowski have outlived them, of course. But the



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good was not interred with their bones. What is good and vital in a contribution to scientific theory is sometimes rapidly transmuted and becomes unrecognizable. The anthropological approaches of today are still the offspring of the old controversies about "structure," "function," and so on.

GREGORY BATESON

Veterans Administration Hospital,  
Palo Alto, California

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1. G. Bateson, *Naven*, 1936, 2nd Edition, Stanford University, 1958.
2. See, for example, John W. M. Whiting, *Becoming a Kuoma: Teaching and Learning in a New Guinea Tribe* (Yale University Press, New Haven, 1941).
3. G. Bateson, "Social planning and the concept of 'deutero-learning,'" in *Conference on Science, Philosophy, and Religion, Second Symposium* (Harper, New York, 1942), pp. 81-97.
4. C. Hull, *Mathematico-Deductive Theory of Rote Learning* (Yale University Press, New Haven, 1940).
5. H. F. Harlow, *Psychol. Rev.* 56, 51 (1949).

The science of anthropology is in great need of thoughtful and serious consideration by trained specialists in the discipline of philosophy. The application of traditional scientific methodology to the social sphere is fraught with difficulties, of which the lack of suitable mathematical techniques is perhaps most prominent. It was no doubt at least partially with this in mind that the Social Science Research Council of Columbia University saw fit to award a grant to Sidney Morgenbesser. Part of the result of this grant was Morgenbesser's article in *Science*.

From such an article one might have expected fresh insights of a constructive nature into the condition of current anthropological theories. Instead, the reader is presented with a number of vague generalities ("many anthropologists claim . . .") which only Morgenbesser would claim to be anthropological theories, to which he applies the most naive of criticisms: "Functionalists are most likely wrong if they mean that every change in every societal institution will produce at least some change in every other societal institution. There were, for example, many Indian tribes (though, to be sure, not all) who incorporated the horse into their way of life without altering many of their habits, customs, and ceremonies." Some empirical research into the subtleties of social change might make Morgenbesser a more competent judge of this position. This is not a matter of being "most likely wrong"; like all scientific statements it is subject to testing and qualification on the basis of empirical data.

Morgenbesser's charge of "banal and possibly beyond disproof" to the functionalist statement that "societal institutions are interdependent in the sense

that changes in some institutions will produce other changes in other institutions," illustrates his essentially biased position. Most accepted scientific theories are based on "banal" foundations. In fact, one indication of the soundness of a scientific theory is the simplicity of its basis.

Again, Morgenbesser says: "The statement that unless certain requirements for group survival are met no group can continue is a tautology. It is equally trivial that in any on-going society, certain institutions or repeated ways of behavior are either necessary or sufficient for meeting these requirements." But functionalists have been asserting for years, never without opposition, that these "tautologies" are, or should be, obvious. And precisely for this reason they are attempting to establish as valid the laws under which these repeated ways of behavior operate. This procedure has been the standard stock in trade of physical scientists for centuries; why does it now come into question only with regard to the field of anthropology?

He continues: "It is not too misleading to recast these observations into teleological language and, with the functionalists [italics supplied], to consider societies as aiming at the solution of certain problems, and to consider institutions as functioning effectively or ineffectively with respect to such solutions." To deny that functional theory is particularly exposed to the dangers of teleology would be folly on my part. But to imply that functionalists are ignorant of such difficulties or that teleology is inherent in functional analysis shows an unbelievable lack of sophistication on Morgenbesser's part. (Interestingly enough, one of his criticisms of learning theory, under the heading "Limitations of Learning Theory," seems to concern its inability to make teleological explanations!)

Anthropologists in general and functionalists in particular were no doubt amused to read Morgenbesser's announcement that "anthropology is threatened with extinction as a logically independent discipline." I suspect that anthropologists are less concerned with creating a "logically independent discipline" than they are with doing their utmost to reveal the wonders of social phenomena. If this involves borrowing theories or adapting to the developments of other disciplines, they will do so, without apology. If this involves following many leads and embracing a multiplicity of theoretical schemes for another hundred years, they will do that too. "The heteronomy of anthropology," which Morgenbesser deprecates, will be its greatest source of strength until sufficient knowledge is obtained to standardize its concepts and methodology. Compare the

situation today with that of the not too distant past, when anthropology labored under the burdens of social evolution theory (a burden now carried by religious historians and philosophers).

To top everything, the tired old proposal that anthropology confine itself to historical statements is presented in a pretentious tone calculated to lend it an aspect of novelty which it lost before Malinowski ("I spit on history"). For an earlier discussion of what Morgenbesser's conclusion merely rehashes, see C. W. M. Hart's charming article, "Cultural anthropology and sociology" [in *Modern Sociological Theory*, Becker and Boskoff, Eds. (Dryden Press, New York, 1957)].

Since *Science* is catering to an audience capable of digesting Ingraham and Green's "Role of magnesium in enzyme-catalyzed syntheses involving adenosine triphosphate," or White's "'Root pressure' in gymnosperms," it has no excuse for publishing an article with a trivial message merely because it concerns social science. It is impossible for a serious discussion of anthropological theory to totally ignore the work of Levi-Strauss (as well as that of many others) as Morgenbesser did, except for the abbreviated versions he found in R. Firth's "Function" in *Current Anthropology* [W. L. Thomas, Jr., Ed. (University of Chicago Press, Chicago, 1956)]. This was an insult to conscientious anthropological theorists. The first 55 pages of *The Structure of Society*, by Marion J. Levy, Jr. (Princeton University Press, Princeton, N.J., 1952) assays most of what Morgenbesser has to say, in detail and thoroughness that surpasses the chemical reports that fill the pages of *Science*. Any introductory philosophy text duplicates whatever Levy ignored.

DAVID E. HANSEN

Class of 1959, Lawrence College,  
Appleton, Wisconsin

My paper was not devoted to the impossible task of reviewing all aspects of anthropology, but to that of assessing some theories currently employed by anthropologists. Bohannan, the most temperate of my critics, has therefore rediscovered my aim when he notes that my treatment was selective, and did not fully consider some important anthropological problems and attitudes. Although Bohannan's sharp distinction between concepts and beliefs seems to me to be debatable, much that he says about the goals of his discipline is instructive. But he does not discuss any theories that can be employed to reach the goals he specifies. Hence Bohannan's paper may be taken as an addition to, rather than as a criticism of, my own.

(Continued on page 347)



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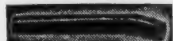
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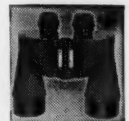
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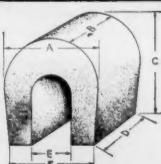
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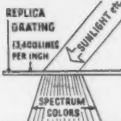
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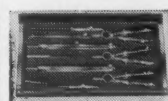
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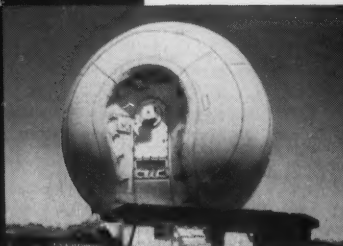


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## Behind the *Times*

Some of the people who read both newspapers and scientific journals are surprised to discover that on occasion new findings revealed by scientists in articles submitted to journals are making their initial appearance in the daily press. Before the scientist himself sets forth in complete form his results and supporting evidence, a reporter scrapes some of the icing off the cake to give the general public a taste. But if this kind of occurrence is one of the signs of the growing role that science is playing in public affairs, it does raise some questions about the functions of the two kinds of publication and of the relation between them. The contrast between newspapers and scientific journals is something more than the contrast between popular and technical presentations.

Although a scientist who has completed an article is anxious to have his findings appear in the near future, to a newspaperman with a story instant publication is none too soon. The interest each newspaperman has in reporting the news before his fellows can work to the advantage of the public and to the advantage of scientists. With a wide range of scientific developments having an immediate bearing on the general welfare and security of the nation, it is important for the public to have news of developments and it is often important that they have the news fast. The scientific community is also served by this form of rapid communication, since investigators can learn of work being done in other laboratories even if the full details are not provided.

If newspapers are primarily interested in quick reporting, then scientific journals are concerned chiefly with establishing the authenticity of what they publish. Of course, a news story about a new finding not only states what is claimed but who makes the claim and the circumstances of his making it. But the appearance of the story in a newspaper, unlike the appearance of an article in a journal, does not add to the authority of the research. Publication of an article in a scientific journal does so because editors of journals frequently have articles describing new work examined critically, before publication, by specialists in the subject with which the article deals.

Although newspapers and scientific journals have different functions, a possible point of conflict between them is that reporters may want information about new findings that scientific editors and publishers should not yet reveal. The only proper way, it seems to us, for a journal to release information about its contents is through the appearance of the journal itself. To institute a policy of disclosing prior to the publication date a portion of the contents would add a task to the work of editing and reviewing articles that might work against the journal's primary responsibility of attempting to insure the merit of what it publishes.

Given the fact that some reporters may ply their trade more cleverly than others, and given the fact that journals are not the only means of communication in the scientific world, another aspect to the relationship between the daily press and the scientific press also follows. Besides scientific journals, there are addresses, meetings, and gossip. Consequently, on occasion a reporter will come upon a piece of research that he finds newsworthy, but which, although it is scheduled for scientific publication, has not yet appeared. If, under these circumstances, the reporter scoops the journal, then those involved in scientific publication hope that he gets the story right.—J.T.





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## CURRENT PROBLEMS IN RESEARCH

### The New American Archeology

Its changing interests are bringing new kinds of understanding and a generalized view of its problems.

Joseph R. Caldwell

It is well known that the fortunes of archeology have been greatly improved by new technical aids such as radiocarbon dating. A more important but far less celebrated advance is represented, I think, by a shift of interest in recent years toward problems of far greater generality than pertain to any single excavated prehistoric site. Part of this shift of interest to more general problems must be ascribed to the outstanding work of V. Gordon Childe and others in the Old World, but the greater reason perhaps is to be found in the close ties which most American archeologists have maintained with general anthropology and through this, more tenuously, with the wider domain of social studies.

This juxtaposition of anthropology and archeology in North American universities came about for the good historical reason that this continent contained living primitive cultures as well as prehistoric ones. The work of Americanists has with reason been called social-science archeology. Not only do many Americanists have a cultural anthropological background but they find fruitful applications in archeological thought of the studies, for example, of social anthropologists such as Julian Steward and Robert Redfield. A series of papers in a recent volume entitled *Seminars in Archaeology: 1955* (1) comprised the following titles: "An archaeological classification of culture contact situations"; "An archaeological approach to the study of cultural sta-

bility"; "The American Southwest: A problem in cultural isolation"; and finally, "Functional and evolutionary implications of community patterning." Had a sociologist found himself at these meetings, he would have had no trouble recognizing the problems, even if the factual data seemed a little strange. An anthropologist writes of these four seminars that they demonstrate "a growing together rather than a falling apart of archaeology and the other special disciplines of anthropology" (2).

#### First Approach

The understanding that is emerging as a result of shifting interests and new problems can be shown by comparing recent archeology with the older variety. Since American archeology is regionally specialized—Andean, Eastern, Middle American, Southwestern, and so on—and because some of these regions were ahead of others in development, I find it easier to use the older archeology of eastern North America as a base line for the kind of contrasts that I propose to make.

Up until World War II the chief concerns of eastern archeology—with some exceptions—seem to have been the description of archeological sites and the description—often simply the definition—of prehistoric cultures. The latter might be presented individually or in terms of culture provinces (3). Sometimes particular categories of material

culture—for example, all the known prehistoric pottery from the eastern United States—were presented in terms of provinces (4). Some fine work was done on the identification and methods of manufacture of prehistoric stone tools (5).

A considerable advance was represented in the Midwest by a Linnaean-like taxonomic system (6) which appeared just at the time it was beginning to be realized that cultural stratigraphy was present in the Eastern areas. The Midwestern taxonomic system was regarded as a necessary first step. It organized archeological materials into categories based on degrees of likeness of the assemblages being unearthed. Unfortunately, there was a tendency to regard classification as the end of research, and some archeologists who were obtaining long stratigraphic sequences, which in some cases showed gradual culture change, were hard put to classify these in Midwestern terms, although they continued for years to do so. In being able, now, to observe material culture changes in time and space, they already have part of the means for establishing kinds of historical connection, whereas the taxonomy they tried so hard to employ could only specify something about degree and could not deal with continuities.

I think it is fair to say that before World War II American archeological studies were in a condition similar to that which Northrop (7) has characterized as the natural-history stage of inquiry. The emphasis was on archeological data as things in themselves rather than on the values offered by different ways of looking at them. Moreover, it was considered, in practice, as important to excavate a site meticulously and to record every scrap of evidence which might conceivably bear on any future problem as it was to have a reason for excavating the site in the first place. One result of all this was the development of a specific kind of problem which treated classificatory entities as independent realities; one might inquire into the content of cultures known from preserved material objects, examine their temporal or spatial boundaries, or try to establish

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the degree of relationship among them.

A second result was the development of a specific kind of analysis to set up the comparisons required to solve problems of this nature. Types were routinely established as an economical means of describing small objects, pottery, constructions of one sort or another, and burial customs. These types were considered adequate for all comparisons which might later be made but were not designed to solve particular problems. Problems might certainly occur to one after the types had been established. Types of this kind, since they were immediately apprehensible regardless of problem, were in some quarters considered to be real entities, and rightly so.

The third result was the development of a specific kind of history—a history of material culture—which, at best, described the succession of the preserved archeological assemblages in each culture province. At worst, such a history was confined to the area of a modern state and made unnecessarily complex by the assignment of different names in different states to cultural manifestations which, on the basis of the criteria in use, should have been assigned the same name.

The essentially dull and uninteresting character of this "culture history" was a matter of concern to some archeologists. Walter W. Taylor (8) called for the construction of fuller cultural contexts—for attention to "the interrelationships which existed *within*" each cultural entity. Others attempted to inject a life-like note by substituting the word *people* for *culture* whenever possible. Thus, in a semipopular book (9), the "Savannah River Culture" became the "Savannah River People," with corresponding shifts in referential pronouns.

### Transition

A trend away from local specialization was initiated in the 1940's in monographs by Ford and Willey in 1941 (10) and by Griffin in 1946 (11). These men made themselves familiar with a vast amount of uncollated and unpublished data which had emerged from the hundreds of excavations undertaken under various federal relief agencies. The prehistory of the eastern United States was found to be most readily susceptible to presentation in terms of a succession of pan-Eastern periods or eras, reflecting the importance of time and continuity in contemporary archeological thought. The

picture obtained from these formulations was of a steady development of material culture and of the essential unity of the East: The various prehistoric cultures assigned to each period in this vast region were usually more alike than were the temporally separated entities within any particular subarea. Hence, each of these major periods could also be regarded as a developmental stage.

In 1958, Willey and Phillips (12) applied what is essentially the same kind of formulation to the whole of the New World, as a series of pancontinental stages. The theoretical foundations of their work were stated at length, some hundreds of areally based prehistoric cultures were characterized, and many special hypotheses regarding the interrelationships of these were proposed, weighed, or discarded. The result of this method was again to show the cultural interconnectedness of the area treated—in this case the entire Western Hemisphere—and to suggest that the civilizations of Mexico and Peru emerged from the same background as the other American cultures but proceeded through additional stages leading to civilization.

American archeology still leans heavily on the idea of areally based cultures and probably always will. We have even improved the utility of this view by the concept of tradition—a culture area having depth in that it is allowed to shift its boundaries through time. If we now suggest some new ways of thinking about areal traditions, this does not mean that we are ready to dispense with them. They do represent more or less closely one kind of natural or common-sense division among the primary materials we have to work with. Where we have improved on the older archeology is by asking different kinds of questions of the materials, and this is directly bound up with the new interests we have noted.

### The New Archeology

The new archeology in America is tending to be more concerned with culture process and less concerned with the descriptive content of prehistoric cultures. There are now two kinds of problems, historical and general, which can be suggested either by distinctions seen in the data themselves or by results of archeological research in other parts of the world, or which can emerge out of other disciplines such as ethnology or philosophy, and then be brought to the data as propositions to be tested.

We may characterize our new interests in the following way. Where formerly we were concerned with the identification of things and of cultures—whether, for example, a particular artifact should be regarded as a knife or as a scraper, or whether a given archeological assemblage should be classed with this culture or that—we have added an interest in the identification of culture processes and situations. Thus, W. R. Wedel's "Environment and Native Subsistence Economics in the Central Great Plains" (13) examines culture-environment connections in that area, and since that time other archeologists, stimulated no less by A. L. Kroeber's "Cultural and Natural Areas of Native North America" (14) than by the fine Viru Valley Project in Peru (15), have turned their attention to the interrelations between natural ecology and human populations and settlement patterns, with respect to cultural level.

Another approach to cultural and historical processes is seen in the wealth of inferences which can be derived from changes in cultural forms seen through time—that is, through stratigraphic and constructed sequences. Whether or not changes were diffused from another region can be inferred from knowledge of whether or not they occurred earlier elsewhere. That changes are of local development can be inferred when their prototypes occur locally at an earlier time. Something about the historical situation can be inferred from rates and magnitudes of changes in cultural forms. A sudden change in a whole series of artifact forms may herald a prehistoric invasion; gradual changes in forms occurring at different times suggests a period of comparative tranquility during which cultural development was not greatly influenced by outside areas. Whereas the older Midwestern taxonomic system could establish degrees of connections among cultural assemblages, we are now finding various methods of inference which will enable us to see the kinds of connections.

Present archeology still reflects an indiscriminate use of the notion of a prehistoric "culture," by which is sometimes meant a few artifacts of some former society and, at other times, a number of societies historically related, but perhaps in different ways and in different degrees. We are increasingly sensitive to the value of making distinctions between cultures as opposed to societies (16). Observations which can be made about behavior are for the archeologist



mediated through cultural forms, but his inferences need not always refer back to culture. Sometimes it is better to use the concept of interaction area instead of culture area; not only is thought thus referred directly to the behavior of people instead of to a "culture," but in some cases this idea is better suited to the archeological facts of continuous intra-area diffusions of cultural forms. In other instances we can make inferences concerning social organization itself (17).

Still another basis for our changing interest stems from the idea of pattern or configuration, which has had a considerable vogue in anthropology although it is not new with that science. The archeologist is inclined to see cultural patterns in developmental terms. A pattern represents some kind of regularity or organization. If a pattern can be recognized, the features we use to account for its presence may perhaps be stated in terms of the processes which brought it into being or perhaps in terms of the factors which operate to maintain it.

With the idea of cultural patterns and developmental patterns, modern archeology has reached a point where many possible patterns and hypotheses can be suggested, each of which seems to propose cultural "facts" that are not necessarily mutually exclusive and that do not necessarily contradict each other but which in the same body of materials reflect various aspects of a many-sided reality. To take a very simple example of the way in which a given body of archeological materials may mirror different historical facts, suppose that a stratigraphic sequence of flint projectile points is used to suggest the answer to the question of whether these points were javelin tips or arrowheads. If both types are present, it may be that the bow and arrow was replacing the javelin during this range of time. We could perhaps arrive at an answer to this problem by using a type system with criteria based upon the size and weight of the specimens. On the other hand, the question might be whether the flint was being obtained from a distance through trade, and for this we should have to examine the projectile points in the light of another type system based on kinds of flint correlated with different localities—not on sizes and weights as in the other case.

In the foregoing example it is relatively easy to see how a given body of archeological materials represents different historical or cultural facts. In the case of cultural pattern or configuration, however, the "reality" of proposed fact

is less apparent because the particular interests of the investigator, and perhaps the historical development of the science, intrude more strongly into the result. Thus, Willey and Phillips' stadial conception of New World prehistory is also concerned with a particular reality; they might have devised other conceptions of equal validity had their interests been other than what they were.

### New Understandings

The views held by Julian Steward, a social anthropologist (18), show how additional understanding has been reached by a different approach. Steward rejects "unilinear" cultural evolution, maintained at the end of the last century by ethnologists like Tylor and Morgan and now in part by Willey and Phillips (19), which says that with certain allowances for diffusion, all human cultures pass historically through similar developmental stages. According to Steward's theory of "multilinear" evolution, all cultures do not pass through similar stages but we can discern a finite number of parallel evolutions in which societies adapted to particular environments and natural resources pass through successive and distinctive levels of "sociocultural" integration. Steward's comparisons deal with societies from various parts of the world. Features of these societies are treated by Steward as types, and certain recurrent associations of important features represent "cross-cultural types."

Conclusions concerning processes involved in particular evolutionary sequences are regarded not as natural laws but as regularities or generalizations of limited range, upon which, one supposes, we may in time build further. Steward says: "Ecological adaptations can be considered as causative in the sense that a degree of inevitability in cultural adjustments is directly observable. Patrilineal bands of Bushmen, Australians, Tasmanians, Fuegians, and others represent a type in that the ecological adaptation and level of integration are the same in all these cultures. In these and other cases, factors producing similar types such as environment, food resources, means of obtaining food, the social cooperation required, population density, the nature of population aggregates, sociopolitical controls, the functional role of religion, warfare, and other features, will have an understandable relationship to one another."

Steward's work is concerned with proc-

esses of culture change manifested in a number of distinct developmental sequences and arrives at generalizations of limited range stated in terms of cultural process, whereas, the Willey-Phillips formulation stresses the interconnectedness of the prehistoric societies of the Western Hemisphere and arrives at a series of cultural levels applying to the area.

Some of Steward's proposed cross-cultural types, such as Formative, Regional Florescent, Empire, and Conquest, are designed to show the processes leading to civilization. They are nearly parallel to the later stages of the Willey-Phillips formulation. Steward's types are now being examined and somewhat modified by archeologists familiar with the various regions (20). The developmental similarities of Steward's types may be stated in causal terms, because between the Old World and the New World there is not much chance that the similarities are due to historical connection.

A new approach sometimes brings a wealth of understanding. Archeology seldom affords direct evidence of social institutions, although Childe has suggested some means by which these can be inferred, and recently Sears has been able to propose a correlation between prehistoric burial mounds on the Gulf Coastal Plain with the presence or absence of strong social classes in the societies involved (21). Now Steward provides another method for arriving at such inferences, as Eggan has pointed out (22). Archeology usually does offer data (for example, the bones of food animals and the size and locations of sites) concerning ecological adaptation. Some social institutions can be satisfactorily inferred from this if, as Steward maintains, they are causally connected with ecological adaptations.

I recently proposed (23) a conception of the development and spread of early civilizations which, like the Steward and Willey-Phillips formulations, rests on a hypothesis. The body of available data is here divided differently, and in thus shifting the focus of our interest, new cultural "facts" are created. According to this scheme, there has been, in the areas which developed civilizations as well as in those which did not, an "Archaic" culture type with certain definable developmental features. These developmental features can be used to account for the emergence of civilizations in some areas as well as for the absence of civilization in other areas. Once a civilization has developed, however, some of the processes involved in its

spread are best seen in terms of a contrast between two additional culture types: "nuclear civilization" and, in the areas outside of civilization, "nonnuclear culture."

The most important developmental feature of the Archaic culture type in eastern North America was the achievement of primary forest efficiency. This was a cumulative process manifested in the development of ambush hunting, in seasonal economic cycles (transhumance), and in the discovery of new sources of natural foods. It is supposed that something like this may have occurred wherever Archaic cultures are found in forested lands. An extension of this idea leads to a definition of a "plains efficiency" for the hunters of large migratory game and a "maritime efficiency" in coastal areas. These various "efficiencies" are meant to be the logical counterparts of "primary farming efficiency"—a term originally used by Braidwood (24) to describe the economic platform upon which civilization may arise.

Plant raising was known in areas where nuclear civilization did not arise. However, it was *only* in areas of nuclear civilization that food production was the economic basis for society. Perhaps the plants used had greater potentialities; perhaps growing populations or the progressive depletion of other resources, or both factors, brought about a Toynbeeian challenge which was successfully met.

In the nuclear civilization culture type, it is the achievement of primary farming efficiency which permits the changes leading to civilization. In the nonnuclear culture area of eastern North America, where primary forest efficiency was well established, it was this very efficiency which tended to direct subsequent economic innovation along lines previously established. Changes only represented further development of hunting-gathering systems.

While a degree of residential stability and comparative freedom from want can be achieved by peoples who live by hunting, fishing, or gathering (witness the American Indians of central California and the northwest Pacific coast), it appears that urbanization and civilization cannot appear without the development of food production on an extensive scale.

The growth potential of different economic patterns is clearly delimited in comparing the nuclear and nonnuclear culture types. The mechanics of the limiting factors can be seen in comparing each of these two with their common antecedents in the Archaic culture type.

What new understanding can be reached by viewing culture developments in the Western Hemisphere in terms of two contrasting types, nuclear civilization and nonnuclear culture? Such a view suggests one way to find connections which became established between the areas of civilization and the areas beyond, and the outward spread of civilizations can be formally examined both in time and in space. It becomes possible to ask certain questions about the spread of civilizations, and although the particular historical events may seem to be of infinite variability, it may be possible to account for these in terms of a finite number of general processes. Within the framework of the contrast between nuclear civilization and nonnuclear culture, it is relatively easy to describe certain intermediate cultural balances as of mixed descent. To do so emphasizes the role of such hybrid cultures as active agents in the spread of civilizations. Finally, it calls attention to the different developmental patterns between the spreading civilizations and the cultures which confront them. An acculturation situation consists of far more than the simple adoption of features of the greater culture by the weaker. Both are affected, and both reinterpret culture transfers in terms of their own views and interests, which we *can* see as patterned in terms of a particular historical development.

### Conclusions

It is supposed that behind the infinite variability of cultural facts and behind the infinite and largely unknown detail of historical situations we shall discover the workings of a finite number of general cultural processes. This hypothesis underlies much of recent archeological thought despite the view, often propounded, that because of level, cultural facts are much more complex than those of the physical sciences. This latter assertion does not make our task impossible. Not all cultural facts are of equal importance in determining a given pattern or trend. Certain developmental patterns must surely be overriding in their effects upon other patterns. A major historical pattern may serve to unite or in some cases to subordinate other patterns of more limited range.

Although, as I have tried to show in this article, cultural facts vary with the hypothesis, and although the hypothesis varies with the special interests of the

investigator, this does not mean that archeological formulations at the pattern level cannot be tested and that some kind of validation cannot be secured. The pathways of archeology are strewn with the wreckage of former theories which could no longer be supported in the light of new data. Some hypotheses are concerned with different aspects of a reality reflected in a single body of materials. There are also hypotheses which can be shown to be logically inconsistent with each other and among which a choice must be made. As time goes on, tests of compendancy will become increasingly specific. Finally, here in the realm of postulated cultural facts there are some from which test cases can be constructed, and in this way the truth of the postulates can be tested. One way to disprove the Willey-Phillips postulate that all the cultures of the New World went through similar developmental stages would be to show that an important area of New World cultures did not go through these stages but did go through others.

I said in the beginning, and have tried to show with reference to the convergence of archeology with anthropology and social studies, that archeology is now turning to questions of greater generality than pertain to any single excavated prehistoric site or culture. I think that our interests will become still wider. The similarities between Steward's views concerning the importance of the food quest in determining the institutions of the simpler societies and Marx' production relationships, which formed the basis for his labor theory of economics, may already have occurred to the reader. V. Gordon Childe apparently found much in Marx' historical formulations to stimulate his own conceptions of prehistory.

Since archeology expects to deal with a range of problems pertaining to former societies and often seeks the aid of other sciences to do this, it tends to make connections among various kinds of studies. Moreover, the appropriateness of archeological data for questions which have arisen in general studies of history or art has long been recognized. Archeological findings from the earth, viewed in terms of time, space, and cultural behavior, offer a vast body of material for inference. And as for philosophy, I think that the usefulness of archeological data will be recognized and that closer connections with that discipline will be established. What does a stratigraphic sequence of changes in cultural forms have to say about the nature of historical causality?

What does the regularity which such changes often show imply concerning historical determinism as opposed to human liberty?

If it is the wise archeologist who now restricts his formulations to the development and persistence of civilizations, cultures, technologies, arts, and lesser matters, it must also be the very dull archeologist who could be unconcerned with the implications of these for some of the perennial problems of Western man.

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#### CURRENT PROBLEMS IN RESEARCH

## Cloud Physics

Not all questions about nucleation, growth, and precipitation of water particles are yet answered.

Henry G. Houghton

Cloud physics is concerned with the condensation and precipitation processes of the atmosphere. In scale, the phenomena studied range from the nucleation of the more ordered phases of water substance and the growth of particles by water-vapor diffusion to the dynamics of the atmospheric processes that lead to the formation of clouds. Although not a new field, cloud physics was given a substantial stimulus in 1946 by the discovery, by Langmuir and Schaefer, of means for the artificial nucleation of ice in supercooled water clouds. In a short article it is not possible to cover all aspects of what has become an active field of research. An effort will be made to point up some of the more recent developments and certain of the intriguing but as yet unanswered questions. The subject of the artificial stimulation of rainfall will be touched on only incidentally, be-

cause an adequate treatment of this still-controversial topic would require an article in itself.

#### Homogeneous Nucleation

A central problem of cloud physics is the nucleation of a new phase—of water from water vapor or of ice from the liquid or the vapor. The theory of homogeneous nucleation in which the new phase appears without the intervention of any foreign substance has been developed by Volmer and Weber (1) and has been expanded upon by others. According to this theory, the appearance of an embryo of, say, water from the vapor is considered to result from the chance aggregation of molecules. If the surface free energy of the embryo is less than the energy released when the molecules aggregate (latent heat), the embryo will persist and become a nucleus; otherwise it will be disrupted into its molecules.

The rate of increase of the surface energy is proportional to the radius, while the rate of release of latent energy is proportional to the square of the radius. Hence, there is a critical radius above which the embryo will persist, and this is given by an equation developed by Kelvin (2).

$$r_0 = \frac{2M\sigma}{\rho RT \ln p/p_\infty}$$

where  $r_0$  is the critical radius,  $M$  is the molecular weight of the liquid,  $\sigma$  is the specific surface energy of the interface,  $\rho$  is the density of the liquid,  $R$  is the universal gas constant,  $p$  is the pressure of the vapor, and  $p_\infty$  is the equilibrium vapor pressure over a plane surface at the absolute temperature  $T$ . Thus, homogeneous nucleation is a probabilistic phenomenon and may be said to occur when the probability of the chance aggregation of molecular aggregates large enough to persist becomes arbitrarily large. From Kelvin's equation and the statistics of molecular aggregation, it is possible, in principle, to predict the conditions under which homogeneous nucleation will occur. Unfortunately, inadequate knowledge of certain physical constants, notably the specific surface free energy of molecular aggregates, to which the equation is very sensitive, preclude definitive quantitative answers. Theory plus experiment suggest that the homogeneous nucleation of the liquid from the vapor occurs only at six- to eightfold supersaturations and that the homogeneous nucleation of ice from the liquid takes place at about  $-40^\circ\text{C}$ . It appears that it is energetically easier for ice to be nucleated from the vapor via

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the liquid phase than directly from the vapor.

From direct observation it is known that condensation occurs in the atmosphere at extremely small supersaturations, and hence it does not seem that homogeneous nucleation of the liquid is involved in the natural condensation process. However, supercooled clouds are occasionally observed at temperatures close to  $-40^{\circ}\text{C}$ ; this and other evidence suggests that the homogeneous nucleation of ice is a factor in the atmosphere. It is of interest here to note that the pellets of solid  $\text{CO}_2$  used for cloud seeding apparently form ice seeds by the homogeneous nucleation of the liquid from the vapor, followed by the homogeneous nucleation of ice in these liquid droplets. In this way a pellet of dry ice 1 cm in diameter may lead to the formation of on the order of  $10^{12}$  ice crystals.

### Condensation on Foreign Nuclei

Nucleation of a new phase of water in the atmosphere occurs more commonly on foreign nuclei. The nuclei of condensation have been studied for many years, since the pioneering work of Aitken (3) and of Wilson (4). In accordance with the scheme of Junge (5), condensation nuclei may be classed in order of increasing size as Aitken nuclei, large nuclei, and giant nuclei. Aitken nuclei have radii of between  $5 \times 10^{-7}$  and  $2 \times 10^{-5}$  cm and are probably formed by condensation or by chemical reactions from gaseous substances. The concentration of Aitken nuclei varies from about 100 per cubic centimeter over the oceans to as many as several million per cubic centimeter in strongly polluted air. Their composition is not known, but it is suspected that many are hygroscopic products of combustion.

A large fraction of the Aitken nuclei carry an elementary electric charge, presumably as a result of collisions with small ions. Ions act as condensation nuclei only at about fourfold supersaturations, and hence the electric charge on the Aitken nuclei is of minor importance in the condensation process.

Large nuclei have radii ranging from  $2 \times 10^{-5}$  to  $10^{-4}$  cm. According to Junge (6), the soluble component of large nuclei is principally ammonium sulphate. Many of the large nuclei consist of insoluble material coated with this hygroscopic salt. The concentration of large nuclei is of the order of from tens to hundreds per cubic centimeter. The giant

nuclei are believed to be composed of sea salts, and their concentration may reach 10 per cubic centimeter near the surface of the sea. Their concentration in continental areas is much smaller near the surface but appears to increase with elevation, as compared with a decrease with height over the sea such that the concentrations over land and sea are much the same at a height of a few kilometers.

The mechanism by which ocean salts are introduced into the atmosphere has been the subject of recent research. Earlier it had been thought that spray from breaking waves was the only source of atmospheric salt particles, but it was difficult to reconcile the apparent worldwide rate of production with the restricted areas of the ocean covered by breaking waves at any one time. Recent studies by Woodcock, Blanchard *et al.* (7) and by Moore and Mason (8) have shown that the bursting of bubbles at the sea surface is an effective means for the injection of drops of sea water into the air.

Droplets are formed in two ways when a bubble bursts; the upper water film shatters into drops and is followed by a vertically ejected jet, which breaks into larger drops. For bubbles a few millimeters in diameter, the drops from the jet are so large that they fall back to the surface, and only those formed by the rupture of the film remain air-borne. The drops formed from the jets of much smaller bubbles are, however, small enough and numerous enough to be an important source of salt particles. Large bubbles are probably produced mainly by breaking waves. A very much larger number of small bubbles form not only in breaking waves but, as shown by Blanchard, also from the air carried into the oceans by precipitation particles and perhaps in other ways. There is also preliminary evidence that electric charge separation occurs during the bubble-bursting phenomenon. Although the concentration of salt nuclei in the atmosphere is relatively small, these giant nuclei may be of dominant importance in the formation of rain in certain types of clouds.

The size, composition, and concentration of condensation nuclei is only one aspect of the much larger and little-studied subject of atmospheric chemistry. There are many types of particulate matter in the atmosphere that do not usually serve as condensation nuclei, and there are many gaseous components. The equilibrium between the various gaseous,

liquid, and solid substances would form the basis for many interesting studies. There is evidence of numerous photochemical reactions in the lower atmosphere. Natural rainfall brings many of these air-borne chemicals to the surface. A simple computation reveals that a square kilometer of the earth may receive on the order of 10 metric tons of soluble substances in the course of a year. Thus, there may be considerable truth in the old saying that snow is the poor man's fertilizer.

The number of nuclei that actively participate in natural condensation is often a small fraction of the total. The largest and most hygroscopic nuclei are activated first, and the total number participating depends primarily on the rate of cooling of the air at the condensation level and thus, in the case of clouds, largely on the updraft velocity. Typically, all of the giant nuclei and most of the large nuclei but few of the Aitken nuclei will be involved. In cumulus clouds the droplet concentration is of the order of a few hundred per cubic centimeter of air, while fogs, in which the condensation rate is much slower, have only a few tens of drops per cubic centimeter. Once the cloud drops are formed they grow by the diffusion of water vapor at supersaturations that rarely exceed a few tenths of 1 percent. The limit to the size they attain is set by the water vapor available for condensation, and, because of the large concentration of drops, they rarely grow by condensation to have a radius much in excess of  $5 \times 10^{-3}$  cm.

The range of drop sizes present in a small volume of cloud has an important bearing on the release of precipitation by one of the processes discussed below. The initial range of sizes is determined in part by the size range of the nuclei, but the condensation process tends to narrow the size range (when measured in terms of drop radii). The observed size range of drops in a small volume of cloud is often markedly greater than would be predicted if the drops grew by condensation under uniform conditions. It has been suggested that the initial rate of condensation will be different in different regions of the cloud and that subsequent turbulent mixing will bring drops of varied history into a given cloud volume. It is also probable that collisions between droplets help to broaden the size distribution. The expansion of moist air in a laboratory cloud chamber leads to the formation of drops of much more uniform size than is typi-

cal of natural clouds; this supports the idea that the broadening is a result of larger-scale processes. Further clarification of this matter is certainly needed.

### The Ice Phase

Attention has been centered on the nucleation of the ice phase in the atmosphere since 1935, when Bergeron (9) proposed his now-classical theory of the formation of precipitation through the appearance of ice crystals in a cloud of supercooled water drops. By virtue of the lower equilibrium vapor pressure over ice than over water at temperatures below 0°C, the ice crystals grow rapidly by vapor diffusion at the ultimate expense of the liquid cloud drops. Subsequent observational and theoretical studies have established the importance of this mechanism. The crucial question is how and under what conditions the ice crystals appear in the supercooled cloud. As already noted, it seems that the homogeneous nucleation of ice in water occurs at a temperature of about -40°C. Observation reveals that ice crystals actually appear in most cases at temperatures from about -10° to -25°C. Further, the operation of the ice-crystal precipitation mechanism demands that the concentration of ice crystals be small compared to that of the cloud drops so that the ice crystals can grow large enough to fall as precipitation; homogeneous nucleation would tend to convert the water cloud to an ice cloud of the same particle size and concentration. It is clear that some type of heterogeneous nucleation (involving foreign nuclei) is operative.

Wegener (10) postulated the presence of sublimation nuclei, by analogy with condensation nuclei, which would promote the formation of ice directly from the vapor. This concept seemed to explain the observations and was widely accepted. It was speculated that natural sublimation nuclei were composed of substances having a crystalline structure similar to that of ice. Subsequently a number of laboratory investigations have been carried out with the purpose of studying the way in which ice is nucleated. A number of different approaches have been used. These have included small expansion chambers of the Wilson type, in which rapid but controlled expansions can be produced; larger chambers in which the expansion rate is slower; diffusion cloud chambers; static cold chambers in which supercooled

clouds are formed; and chilled surfaces arranged so that the temperature and vapor pressure may be carefully controlled over extended periods. The results, although somewhat varied, show that in the great majority of cases ice crystals appear only when water saturation is attained. In only a few instances have ice crystals been observed when the vapor pressure is below water saturation but is supersaturated with respect to ice. These results have been interpreted as showing that the ice phase is attained only through the liquid phase. The occasional appearance of ice below water saturation is attributed to condensation on hygroscopic mixed nuclei or on microporous nuclei below water saturation. These conclusions are supported by the knowledge that the interfacial free energy between a solid particle and supercooled water is less than that at the solid-vapor interface. The nuclei responsible for the appearance of ice particles are properly called freezing nuclei if they nucleate ice from the liquid. Because of uncertainties about the process involved, some meteorologists prefer to use the term *ice-forming nuclei* or simply *ice nuclei*.

The concentration of ice particles that appear in natural surface air increases rapidly as the temperature is decreased. Typically, one crystal per cubic meter (about the limit of detectability) may appear at temperatures of from -10° to -15°C. At about -20°C there may be ten crystals per liter, and at -30°C a very large number, approaching 1000 per cubic centimeter, may be observed. At any given temperature the concentration of ice crystals is found to vary by several orders of magnitude from one day to another. Computations of the rate at which ice crystals must form to explain the observed precipitation rates lead to a requirement for concentrations of the order of 100 per liter. This would appear to require temperatures of the order of -20° to -25°C, but observations show that snow often forms at much higher temperatures. It has been suggested that ice crystals may provide additional seed crystals by shedding tiny splinters as they fall. Such a process has been demonstrated in the laboratory, though it is difficult to imagine that the more solid forms of snow crystals, such as unbranched plates and columns, could participate in this process.

The cloud chamber experiments discussed above do not reveal the nature of the natural ice nuclei but only the temperatures and vapor pressures at which

the cloud chambers nucleate ice. Because of the minute size of the nuclei, their composition has been studied principally by adding nuclei of known composition to cloud chambers or to purified water drops. Much of this work was stimulated by the discovery by Vonnegut (11) that silver iodide crystals were effective as ice nuclei at temperatures lower than about -5°C. Vonnegut chose silver iodide on the basis of the close quantitative agreement between the crystal lattice dimensions of silver iodide and of ice. Subsequently the activity of a large number of chemicals, natural soils, and dusts has been studied.

The results have been diverse and often confusing. Most investigators agree on the activity of AgI, PbI<sub>2</sub>, HgCl<sub>2</sub>, CuS, CuI, and I<sub>2</sub>. It also seems clear that many clay (silicate) minerals are effective as ice nuclei. A wide variety of other substances, some soluble in water, have been reported to be effective by some investigators and ineffective by others. It is suspected that some of the disagreement has resulted from contamination of the materials, either as obtained or during the experiments. Bigg (12) found that the freezing temperature of water drops was raised by the addition of small amounts of HI, in distinction to the depression of the freezing point caused by most soluble inorganic substances. Isono (13) has suggested that the ice-nucleating effect of AgNO<sub>3</sub> or of other soluble substances is due to the salt acting first as a condensation nucleus, followed by freezing after condensation has led to the appropriate dilution. The insoluble ice nuclei, such as AgI, PbI<sub>2</sub>, CuS, and kaolinite, have lattice parameters that are reasonably close to those of ice. It has been postulated that ice crystals may form on such host crystals by oriented overgrowth, sometimes called epitaxy. It is expected that continuing research will confirm this mechanism.

Current knowledge thus suggests that there may be three distinguishable types of ice nuclei. These are the insoluble crystalline substances on which ice may form by oriented overgrowth, certain soluble substances which act in an unknown way in the liquid at suitable concentrations, and insoluble particles that act when immersed in the liquid. Of the substances studied in the laboratory, the clay minerals are most likely to be present in the atmosphere in suitable concentrations. This has been partly confirmed by Kumai (14) and by Isono (15), who used the electron microscope

and electron diffraction to identify the nuclei of natural snow crystals. The great majority contained soil particles, and Isono was able to identify 15 of 20 as clay particles. Mason and Maybank (16) have found nine natural silicates and a number of other minerals active as ice nuclei at temperatures between  $-5^{\circ}\text{C}$  and  $-17^{\circ}\text{C}$ . They also found that about half of these substances became active at significantly higher temperatures when they had served as nuclei once and the ice had been evaporated at temperatures somewhat below  $0^{\circ}\text{C}$ . This preactivation may serve to explain the formation of ice crystals in the atmosphere at temperatures higher than the initial activation temperatures of natural particles.

Bowen (17) claims to have shown that there are certain calendar dates on which the total rainfall over a long period of years and in all parts of the world is significantly greater than on adjacent dates. He ascribes this to ice nuclei injected during meteoric showers and arriving in the lower atmosphere about 30 days later. Mason and Maybank (16) found one sample of powdered stony meteorite that served as ice nuclei at  $-17^{\circ}\text{C}$ , but there is still no adequate evidence to support Bowen's interesting hypothesis. One of the most difficult

points to explain is the remarkably constant time of  $30 \pm 1$  days for the particles to reach the cloud level from the outer atmosphere.

### Growth of Ice Crystals

Once ice is nucleated in the atmosphere it grows by diffusion of the vapor and by the accretion of supercooled cloud drops. The former process leads to the formation of a variety of crystal forms, while the latter tends to produce irregular masses. The beauty and complexity of form of snow crystals has attracted the attention of artists for many years. Ice crystals may be broadly classified into hexagonal plates, stellar crystals, columns, needles, spatial dendrites, capped columns, and irregular particles. Within each classification there exist many varieties, and it has been said that no two crystals are exactly alike. All of these crystals are formed in a temperature range of about  $50^{\circ}\text{C}$  and with vapor pressures between ice and water saturation. No other substance exhibits such a wide variety of crystal forms within such a narrow range of ambient conditions.

Experimentation has made it clear that temperature plays the dominant role in determining the crystal habit, the degree of supersaturation with respect to ice being of secondary importance. As the temperature is reduced, the general habit changes from plate to column to plate to column. Needles are formed in a narrow temperature range around  $-5^{\circ}\text{C}$ ; dendritic forms, around  $-15^{\circ}\text{C}$ . The transition from one form to another is very sharp, one or two degrees often being sufficient to cause a change of form. One is struck by the fact that the precedent form has no effect; plates may be caused to form on the end of needles, or dendrites to sprout from plates, simply by properly changing the temperature. Supersaturation over ice governs the rate of growth, and there are limiting supersaturations below which a particular form will not occur, but even changes of several hundred percent make no major differences in the crystal form. Photographs taken by Mason (18) in a diffusion cloud chamber (Figs. 1 and 2) graphically illustrate this remarkable property of ice crystals. On the other hand the presence of traces of organic vapors (even the low vapor pressure of the silicones) produce marked changes in crystal habit. No satisfactory explanation has been found for this great

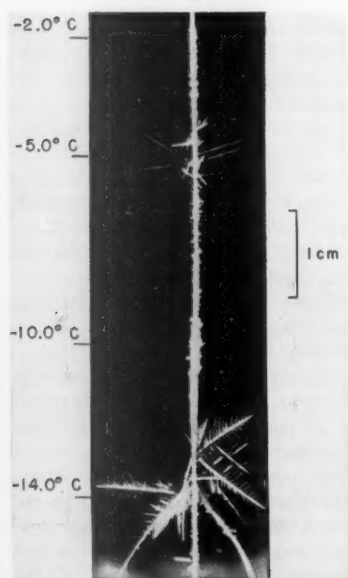


Fig. 1. Ice crystals grown in a diffusion cloud chamber in which the temperature varies with height, as indicated on the scale at left. Note the needles at  $-5^{\circ}\text{C}$  and the dendritic crystals at  $-14^{\circ}\text{C}$ . [Photograph furnished through the courtesy of B. J. Mason (18)]

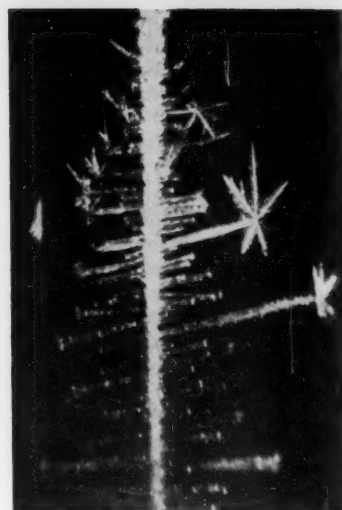


Fig. 2. Needles which were grown at about  $-5^{\circ}\text{C}$  in the diffusion cloud chamber developed stellar crystals on their ends when transferred to a temperature of about  $-15^{\circ}\text{C}$ . [Photograph furnished through the courtesy of B. J. Mason]

sensitivity of crystal habit to temperature. It is believed that the changes observed must be due to alterations in the surface structure and properties of the crystal faces, but no real clues have yet come to light.

### Precipitation Mechanisms

Cloud is the precursor of rain, and it was once thought that raindrops formed through continuation of the process of cloud condensation. It has already been noted above that the limit to the size attained by cloud drops is set by the total vapor available and the concentration of the drops. It is often argued that the limit is set by the time required for growth to raindrop size by condensation, but this overlooks the fact that ice crystals have larger masses than cloud drops because of their lower concentration. In any event the mass of a typical raindrop is about one million times that of an average cloud drop, and it is abundantly clear that processes other than condensation on cloud drops are involved in the formation of rain. Two processes are known which, in combination or separately, seem to be capable of explaining the release of rain and snow from clouds. One of these is the ice-crystal process which is attributed to Bergeron (9), although Wegener (10) presented the basic idea much earlier.



This process envisages the appearance of a number of ice crystals in a supercooled water cloud either by nucleation or by the intrusion of crystals from a higher level in the atmosphere. The ice crystals will grow at the ultimate expense of the cloud drops, and, if the concentration of the ice crystals is several orders of magnitude smaller than that of the cloud drops, the former may attain a size sufficient to fall as precipitation particles.

The second precipitation mechanism is the accretion or collision process, which was first seriously proposed by Houghton (19), although others had previously discussed the possibility and rejected it as contrary to experience. This process is based on the relative velocities of fall and of the consequent collisions to be expected in a cloud of drops of nonuniform size. The ice-crystal process is operative only in supercooled clouds and is most effective at a temperature of about  $-15^{\circ}\text{C}$ . The accretion process operates at any temperature, although there are differences depending on whether the particles are liquid or solid. The rate at which precipitation elements can form and grow by accretion depends on the initial range of particle sizes (particularly on the size of the largest drops), on the concentration of drops, and on the sizes of the collecting and collected drops.

It is necessary to examine each precipitation mechanism quantitatively to ascertain that it is capable of forming precipitation particles of commonly observed size under the conditions that obtain in the atmosphere. Ice crystals in a supercooled water cloud grow by diffusion, and the basic equation for this process is well known. The difficulties result from the complex shapes of the ice crystals and from the effect of the fall of the crystals through the atmosphere. Substitution of simple shapes, such as a circular disc or an ellipsoid of revolution, as suggested by Houghton (20), gives results that compare favorably with such experimental data as have been obtained. Some empirical data for spheres are available on the effect of the velocity of fall on the growth rate. These are of doubtful applicability to ice crystals, but nothing better is available. It is not likely that computations of growth rates of ice crystals made on the basis of these approximations are in error by more than a factor of two.

The accretion process seems disarmingly simple on first sight. Through

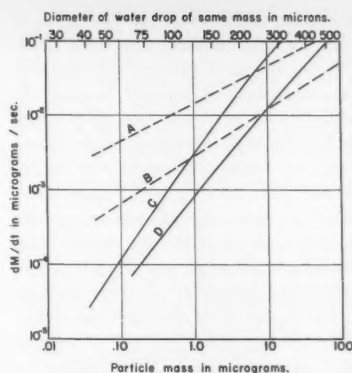


Fig. 3. Rate of growth of precipitation particles by diffusion on ice crystals (dashed lines) and by accretion of liquid drops (solid lines) versus mass of the particle. (A) Stellar crystal at water saturation at  $-15^{\circ}\text{C}$ ; (B) hexagonal plate at water saturation at  $-5^{\circ}\text{C}$ ; (C, D) growth by accretion of liquid droplets in two clouds of different observed drop-size distribution.

knowledge of the velocity of fall of the drops and their concentration, the growth rate of the largest drop by collision with the smaller ones is easily computed if it is assumed that all of the drops follow linear, parallel paths. It was soon realized that the aerodynamics of the process would cause the trajectories of the small drops to be diverted from those of the larger drops and that the "catch" would be less than that predicted by the simple linear motions. This problem was solved numerically for all cases in which the small drop could be assumed to be a mass point; such solutions apply when the collecting drop is much larger than the collected drops. The collection efficiencies, defined as the ratio of the actual number of collisions to the number predicted from linear trajectories, were found to range between 0.5 and 0.9 under typical conditions.

When the collected and collecting drops are both small and of about the same size, as would be the initial situation in a cloud, the aerodynamics are more complex, since the changing flow pattern around the pair of drops must be considered. No analytic solution has been obtained, but it is observed that the overtaking drop is diverted towards the wake of the smaller drop, and that this results in collection efficiencies greater than unity. It has also been observed that two small droplets on a collision course usually do not coalesce but only undergo an elastic collision. This behavior, which is poorly understood, is profoundly modified by the presence of

a vertically oriented electric field, as shown by Sartor (21). This has led to suggestions that the electric field that appears in most pronounced form in a thunderstorm may be an important cause for the release of precipitation, rather than simply an effect. In any event the effect of the electric field and presumably of droplet charge on the coalescence process shows that electrification processes in clouds must be included in any complete study of the precipitation mechanism. Space does not permit a discussion of the current status of knowledge on atmospheric electricity in this article.

In spite of the uncertainties touched on above, it is possible to gain an important insight into natural precipitation processes from the approximate computations that can now be made. Examples of such computations are presented in Fig. 3. This shows the rate of growth of ice crystals by diffusion and of liquid drops by accretion as a function of the mass of the growing particle. When the particles are small, the rate of growth of the ice crystals is much greater than growth by accretion. For larger particles the reverse is true. The two mechanisms are equally effective at a particle mass corresponding roughly to drizzle drops. Clearly, the most efficient precipitation process would be one that started with growth of ice crystals, followed by growth by accretion. As will be seen later, nature often appears to follow just this pattern.

## Natural Precipitation Processes

Clouds may be classified into two broad types, which will here be called convective and stratiform. Convective clouds are formed by rising bubbles and columns of warm air; this process results in the various forms of cumulus clouds, from the small fair-weather variety to the thundercloud. Stratiform or layer clouds result from large-scale lifting processes that may extend over several thousand square miles. They are usually associated with the migratory storms or low-pressure areas of middle latitudes. The stratified form reflects the moisture stratification that is almost always found in the atmosphere except when it is violently stirred by penetrative convection. convective clouds that produce precipitation are characteristically of large vertical extent, have a small horizontal cross section, and have a life of the order of an hour. Stratiform

clouds are typically of great horizontal extent, are relatively thin, and have lifetimes of the order of days. One of the most important differences is that the vertical air velocities in convective clouds are of the order of meters per second, while such velocities are only of the order of centimeters per second in stratiform clouds. This is not only a difference of two orders of magnitude; it spans the range of falling speeds of cloud particles and precipitation elements. In convective clouds only the large precipitation particles can fall with respect to the ground; all others will be carried up by the vertical air currents. This means that the precipitation process must start in the lower part of the cloud; otherwise the particles would be discharged from the cloud top before they reached a size sufficient to survive descent outside the cloud. On the other hand the gentle updrafts in the stratiform cloud are barely sufficient to sustain the cloud particles. In this case the precipitation process must begin in the upper part of the cloud or clouds.

Much interest has centered around the question of whether the ice crystal or the accretion mechanism is dominant in convective clouds. In low latitudes there is now ample evidence of rain falling from clouds all parts of which are warmer than 0°C. This shows that the accretion process acting alone can produce rain. It is in these regions over the tropical oceans that sizable numbers of large sea-salt nuclei have been observed, and the large cloud drops formed on them favor the accretion process. In middle latitudes the situation is not so clear. In most cases a convective cloud deep enough to produce rain extends above the 0°C level, thus making the ice phase possible. As noted above, the precipitation process begins in the lower and hence warmer part of the cloud, and ice is not likely to be involved unless the cloud base is very high and cold. This has been largely confirmed by radar observations of the first echo in convective clouds in the Midwest and also over England. In the majority of cases the first echo appears at a level where the temperature is near or above 0°C. It would not be expected that ice would appear at temperatures warmer than -10° to -15°C.

As the growing particles are carried higher, they will freeze, but it is believed that even then they grow mainly by accretion because of the high drop concentrations typical of convective clouds.

Accretion under these conditions forms graupel or, in extreme cases, hail. Actually the accretion process is favored by the formation of graupel because its bulk density is much less than unity (0.125 has been measured) and therefore the capture cross section is increased. Thus, the present impression is that the accretion mechanism is the dominant one in convective clouds. That this is not always so is indicated by the appearance of convective snow showers in the winter. This makes it clear that under certain conditions the ice-crystal mechanism can operate in convective clouds.

Many more observations in the free atmosphere will be required to really clarify the details of convective precipitation. A cumulus cloud may appear to be a relatively simple phenomenon, but we have much to learn about its dynamics as well as about the distribution of ice and water particles before we can speak with confidence about the way in which precipitation is released. Radar has proven to be a valuable tool, but there is no substitute for properly instrumented aircraft, and very few have been available for meteorological research.

Substantial amounts of precipitation seldom fall from a single layer of stratiform cloud. There are commonly several layers of cloud separated by cloud-free air. The upper layers are often composed of ice crystals, and there may be both supercooled and warm water clouds at lower levels. It should be emphasized that there is no standard arrangement of such clouds and that, in a single storm, there are marked differences from one region to another. It is believed that the precipitation forms as ice crystals, which grow by the diffusion of water vapor. Growth is enhanced if the crystals fall through a supercooled layer, and they may accrete a number of drops, but not to the extent of forming graupel. If the surface temperature is near or below 0°C, the crystals reach the surface as snow. Individual snow crystals seldom attain a mass greater than that of a drizzle drop. Even casual examination will show that individual crystals seldom reach the ground; instead, the "snow flakes" are commonly aggregates of many individual crystals. This is a type of accretion about which we know very little, but it is clearly of considerable importance. It was thought that such aggregation was most probable near 0°C when it was believed that the crystals must be wet in order to stick.

Some recent experiments, by Hosler *et al.* (22), with ice spheres has shown that they will stick at temperatures as low as -40°C if the atmosphere is saturated with respect to water and as low as -25°C at ice saturation. The mechanism is not understood, although it has been suggested that there is a film of liquid on the ice surface even at quite low temperatures. Here again is evidence that we need to know much more about the surface properties of ice.

If the temperature at the ground is above 0°C, the snow which is formed aloft will melt and reach the surface as rain. In order to explain raindrops of the observed size, it is necessary to assume that considerable aggregation of the ice crystals occurs before they melt. Often a low, warm cloud is present in which these melted aggregates can grow further by accretion before they reach the ground. The dominant mechanism in stratiform precipitation appears to be the ice-crystal process, but accretion in the form of crystal aggregation or accretional growth in lower warm clouds must be invoked to explain the observed size of the precipitation particles reaching the ground. Convective elements are often imbedded in predominantly stratiform systems. These may appear at relatively high levels, releasing streamers of snow which may grow further at lower levels by some of the processes already mentioned. As in the case of convective precipitation, we possess only a skeleton knowledge of stratiform precipitation processes. Here again radar and instrumented aircraft are the most promising research tools.

## Conclusions

One can be quite confident that the ultimate success of methods for the artificial release and control of precipitation will depend on the acquisition of much more complete basic knowledge of the ways in which nature produces rain and snow. Present attempts at rain-making are often uncomfortably close to shooting in the dark. This is not to say that nothing has been learned from or accomplished by these efforts, but simply that the effective control of any mechanism demands much more complete information than is now available on natural precipitation.

It is hoped that this brief article will give the reader some insight into the current knowledge and unsolved problems of cloud physics. The solutions will

require the application of a variety of scientific disciplines, and it is hoped that scientists other than meteorologists will be encouraged to tackle some of the challenging problems. There is need for both laboratory and field research. The tools that have been applied are in many instances inadequate. This applies particularly to the difficult problem of measuring cloud-physics parameters from aircraft. Not only is the subject of cloud physics an interesting scientific study but it bears directly on the increasingly pressing problem of the fresh-water supply of the world.

## Alvin Seale, Naturalist and Ichthyologist

Alvin Seale, adventurer, explorer, field naturalist, ichthyologist and aquarium expert, soldier of fortune, and one of the last of David Starr Jordan's personal disciples, died recently at his home in Corallitos, California, aged 87 years and 20 days.

This Indiana Quaker farm boy rode his bicycle across the country to enter Stanford in 1892. He was an outstanding zoology student and in the usual course of events should have graduated in 1896. But he was an unusual student, and his field trips often caused great gaps in classroom work. In 1896 Jordan selected Seale and Norman B. Scofield to go to the mouth of the Mackenzie River, British America, to see if there were salmon there. This was Seale's first trip to Alaska, Point Barrow, and the Arctic Ocean. His unpublished narrative of this trip is very interesting. Another year he collected sea birds on the Alaskan coast for the British Museum. Then he and his roommate joined the gold rush to the Klondike. His companion "struck it rich," but Seale was too busy studying animals in the wild to bother with panning gold. In his diary he says, "an exciting year."

He went back to Stanford in the fall of 1899, only to leave for Honolulu when appointed field naturalist for the Bishop Museum. In 1900 he made the first zoo-

logical survey of Guam, returning via Manila, Hong Kong, China, and Japan. From then until September 1903 he collected all over Polynesia. He explored the Society Islands, the Tuamotu Archipelago, and the Marquesas, Gambier, Austral, Cook, and Samoan islands. He visited, in turn, New Zealand, Australia, the New Hebrides, and the Solomon Islands, returning to Australia several times. These South Sea years were filled with many rare experiences. Just a few of them were related in his privately printed book, *Quest for the Golden Cloak*.

He returned again to Stanford in the fall of 1904 and graduated the following May, 13 years after matriculating. During those 13 years he had come to know more about Polynesia and about its fishes and fisheries than anyone else in the United States and had published a creditable number of important papers.

His next adventure stemmed from his being sent by Jordan to the Texas coast to collect cyprinodonts and carry them to Honolulu to combat the mosquito pest. One of them, *Gambusia patruelis*, was an eager destroyer of mosquito larvae. During the next two decades it was distributed to warm countries all round the globe and was of great help in fighting the mosquito plague.

In 1906 Seale was put in charge of

the Anna Alexander Expedition to Alaska. Bears, moose, bighorn sheep, and other mammals were collected for the Alexander Museum of Vertebrate Zoology at the University of California.

In February 1907 President Theodore Roosevelt appointed Alvin Seale fishery expert for the Philippine Government. The next 10 years were spent in this attractive and stimulating position with the Philippine Bureau of Science. Some of the important things accomplished were the following: collecting, studying, and publishing on the little-known fishes of the islands, emphasizing those of economic importance; publishing the first study of the very important bangos fishpond industry; publishing the first study of the valuable pearl and pearl-shell industry, mapping the pearl oyster beds, and drawing up laws to regulate the industry; publishing the first studies of the island sponges, mapping the sponge reefs, and developing a profitable sponge industry; developing a pearl-button industry through studies of shells suitable for the purpose, and drawing up laws to regulate the industry; demonstrating the feasibility of canning the high-quality sardines so plentiful in the Philippines; publishing upon the wealth of other marine shells and developing methods of utilizing them commercially; publishing studies upon sea cucumbers and the trepang industry; publishing upon other marine resources and suggesting how to utilize them; and publishing on the fishes of Hong Kong and of Borneo. Seale brought carp from China and introduced them to the rivers of Mindanao; he brought black bass from the United States and planted them at Baguio, northern Luzon, at an altitude of 5000 feet; he brought mosquito fish from Honolulu to Manila and planted them in various parts of the Philippines;

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Alvin Seale at his desk when he was superintendent of Steinhart Aquarium in San Francisco.

and he drew plans for an aquarium, supervised its construction, and made the Manila Aquarium the most outstanding one in the world for colorful tropical reef fish. During these years he visited Formosa and made several trips to Borneo, Hong Kong, China, and Japan.

In 1917 Seale resigned and went as ichthyologist to the Harvard Museum of Comparative Zoology. In 1920 he retired and settled on his ranch near Santa Cruz, California. In 1921 he was induced by B. W. Evermann to revise the plans for the Steinhart Aquarium in San Francisco, supervise its construction, and become its superintendent. This post he held for 20 years, resigning when

he reached 70. In 1929 he revisited Samoa to collect fishes for the aquarium. In 1931 and 1932 he was put in charge of the scientific staff of the G. Allan Hancock Expedition to the Galapagos Islands. In 1935 and again in 1939 he made trips to the Hawaiian Islands to collect reef fishes for the Steinhart Aquarium. In 1938 he and his wife made a trip around the world. In 1941 they visited the Yukon, Fairbanks, Mt. McKinley, and Seward, Alaska.

Alone or in collaboration with others, Seale was the author of 162 books and pamphlets, some of them of much scientific importance.

Alvin Seale was more successful than

anyone else I have ever met in transporting live fish from a cool climate to the tropics, or from high altitudes to sea level. Only those of us who have had experience in such difficult matters can appreciate his phenomenal skill in transporting live fish to alien habitats. He had none of the aids which now reduce the risks to almost nothing.

Alvin Seale was twice married. His first wife, Ethel Prouty, whom he married in 1908, died in 1936. In 1938 he married Jessie Frapwell, who survives him.

For many years the hobby of the Seales was collecting books on the South Seas. Ultimately this library of 1300 volumes contained practically all the rare chronicles of early voyages and expeditions. In 1936 Seale donated this valuable collection to the city library of Pacific Grove, California, as a memorial to his first wife.

Seale was a man of the highest character, smiling and gracious, who made friends wherever he went and held them for life. In the 55 years I knew him I never heard him say an unkind or slanderous thing about anyone. He exemplified the principles of his Quaker parents.

After his retirement, the Seales lived at their lovely home in Corallitos. Here a stream of friends from all over the United States and the countries of the Pacific came to see Alvin Seale. The hospitality of the Seales became legendary, and to be invited to one of their house parties was a great privilege.

I pay tribute to Alvin Seale not only as a scientist but also as a man who enriched the lives of his family, his friends, and his associates.

ALBERT W. C. T. HERRE  
*College of Fisheries,  
University of Washington, Seattle*



# News of Science

## Federal Budget Sets Research and Development Funds at \$5.5 Billion with Emphasis on Space Activities

The budget of the United States Government for the fiscal year ending 30 June 1960 has been submitted to Congress by the President. It is balanced, precariously in the opinion of many commentators, at \$77 billion. If Congress responds to it as President Eisenhower would like, income will exceed expenditure by the insignificant sum of \$70 million. The projected surplus is based on the acceptance by Congress of a number of proposals for increasing revenue, among them a second increase in postal rates. Criticism of the budget by members of the Democratically controlled House and Senate has been caustic.

Funds allotted to the Government's scientific programs are the highest in the country's history. The projected figure for research and development for the coming year, \$5.5 billion, exceeds the 1940 expenditures by \$5.4 billion, the 1950 expenditures by \$4.4 billion. Although the research programs undertaken or continued with these funds will cover virtually the whole range of scientific and technological activity, the emphasis will be on space activities as conducted by the military agencies, particularly the Advanced Research Projects Agency, and the new National Aeronautics and Space Administration.

### Scientific Agencies

The Department of Defense, in this and recent previous budgets, has received the greatest share of the research and development budget. This is a reflection of the fact that "major national security" accounts for \$45.8 billion, or 59 percent of the total budget.

The Defense Department, for its research, development, test, and evaluation programs, is authorized a total of \$3.38 billion, of which \$415 million goes to the Advanced Research Projects Agency. The figure for these programs in 1958 was \$2.18 billion. The estimated figure for the Defense Department for the current fiscal year, which will end 30 June, is \$3.17 billion. No figures are available for the original requests of the Defense Department, but the assumption can be made that the allocations fall much below the requests. Observers note,

for example, that the Army received only half of its request for weapons modernization.

The federal budget, a document of 1105 pages, contains a special analysis section which brings together various governmental programs—aid to state and local governments, statistical programs, and others. One such section analyzes federal research and development programs. In this section no distinction is made between the National Advisory Committee for Aeronautics and the National Aeronautics and Space Administration which absorbed it. Thus, the \$280 million that NASA is authorized for research and development for 1960, its first full fiscal year, may be compared with the figures allotted in the past to NACA. In 1958 the actual figure for expenditures was \$89.2 million; in 1959, the fiscal year during which NASA superseded NACA, the figure is an estimated \$153 million. One incidental point can be made here. The budget for fiscal 1958 estimated the expenditures of the National Advisory Committee for Aeronautics at \$86.5 million. The actual expenditure for that year as given in the current copy of the budget was the figure above, namely \$89.2 million. The disparity between the estimated and actual figures can be expected to be the case in regard to many elements in the Administration's budget for 1960. The balance, as the commentators suggest, is precarious indeed.

The research and development program of the Atomic Energy Commission is carried on in contractor-operated laboratories, in universities, in private research institutions, and by industrial contractors. The commission's expenditures for this program for fiscal 1960 have been set at \$651 million. The fiscal 1959 estimate is \$617 million. A total of \$516 million was actually expended in 1958. Elements of the research and development program of the commission are production and weapons, reactor development, physical research, biology and medicine, and isotope development. Until Congress reviews the budget, the original request of the commission for these aspects of its work will not be

known. As in the case of the Department of Defense, it can be assumed to have been higher, perhaps by one quarter or more. The standard attitude of a governmental agency is that the figure in the budget is its official request until later hearings before Congress bring out its actual request.

The major research expenditures of the Department of Health, Education, and Welfare are under programs of the Public Health Service working through the National Institutes of Health. The seven institutes have been authorized a total of \$216.8 million by the Budget Bureau for the coming fiscal year. The total Public Health Service budget for research and development stands at \$236.5 million. The institutes received \$154.4 million in 1958 and have an estimated budget of \$206.2 million for the fiscal year now in progress. For the past 7 years Congress has appropriated more funds for the NIH than the Administration has recommended. Should this trend continue, especially in the magnitude of the increases—39 percent for fiscal 1959—there is reason to believe that the actual budget for research and development for NIH may be on the order of 300 million.

### Congressional Review

The Administration has given its definition of the proper role of the country's scientific community for the coming fiscal year. Its budget is the shaping tool. There is a widely held opinion that Congress, led by Senator Johnson, who had a major role in the establishment of the National Aeronautics and Space Administration, will vote very considerable increases in funds to many of the government agencies, particularly those concerned with national defense. The Congress will thus offer another definition of the scientific community's role. When it comes time to review the budget, many of the questions that will be asked will reflect the kind of thinking that had a full expression in a report of the President's own Scientific Advisory Committee. This report, *Strengthening American Science*, which called for a federal council for science and technology, examines in depth the needs of the nation's scientific effort. It points out the critical role that basic research has in any scientific program and maintains that basic research in the United States is insufficiently supported. Two quotations from the report illustrate the committee's approach on this point. "Research and development" is an elastic term that covers a broad spectrum of activities. . . . "Of the government's total research and development budget, it is estimated that about 6 percent is earmarked for basic research." In his letter covering this report the President said that "the task of

further strengthening United States science is so broad that government, industry, universities, foundations, and individuals all have essential roles to play." Congress will ask, when it reviews the new budget, whether the government has fully accepted its "essential role."

### Improving High-School Education

During January the national concern about high-school education manifested itself in a number of significant ways. Of first importance was the release of James B. Conant's report, *The American High School Today*. With the support of a Carnegie Foundation grant administered by the Educational Testing Service, Conant made a close study of 55 high schools in 18 states that led to 21 specific recommendations. Conant says of his work, which is to continue for at least another year:

"I can sum up my conclusions in a few sentences. The number of small high schools must be drastically reduced through district reorganization. Aside from this important change, I believe no radical alteration in the basic pattern of American education is necessary in order to improve our public high schools. . . . I think one general criticism would be in order: the academically talented student, as a rule, is not being sufficiently challenged, does not work hard enough, and his or her program of academic subjects is not of sufficient range. . . . A correction of this situation in many instances will depend upon an altered attitude of the community quite as much as upon action by a school board or the school administrators."

#### Columbia Honors Program

New York City has recently demonstrated the effectiveness of the community interest to which Conant refers. This fall a science honors program was established at Columbia University's School of Engineering with the aid of \$29,000 from the Hebrew Technical Institute of New York and \$25,000 from the Fund for the Advancement of Education of the Ford Foundation. The first half of the program has just been completed, and an initial report indicates that it is a complete success. The professors responsible for the 158 students, 25 of whom are girls, describe the students' ability with unqualified enthusiasm.

Every Saturday morning the group hears lectures on such subjects as physical and chemical metallurgy, theory of vibrations and wave motions, symbolic logic, nuclear physics, protein chemistry, and population genetics. Laboratory work includes advanced projects in enzyme chemistry, chromatography, effects of radiation on soils, and animal behavior.

Every week 50 students have lunch at the men's faculty club, where five students are seated with each faculty member.

John R. Dunning, dean of the engineering school, says that the program is intended "to make the best science available to the best minds and to set an example that all colleges and school systems can follow."

#### Nationwide Survey of Students

Another important event in public high-school education to be reported last month was the U.S. Office of Education's national student survey, a survey that would attempt to assess the quality and quantity of these "best minds." The project would include a program of tests that would cover aptitude, achievement, personality, interest, and biographical data. These tests, given to a 5-percent sampling of the nation's high-school students (500,000), would provide a representative picture of the entire high-school population. The Office of Education's Research Advisory Committee has already approved the planning phase of the project, for which \$335,000 has been provided. However, final action on the plans will not be taken until the committee meets in mid-February. If the study is approved—and it would cost more than \$1 million—work will begin on 1 March. In May, a 2-day series of 25 tests would be given to 1000 to 1500 pupils as a pilot study; the national testing would be done next January and February.

The survey has been planned cooperatively with the National Institute of Mental Health, the Office of Naval Research, and the National Science Foundation. The project would be conducted by the American Institute for Research, a nonprofit affiliate of the University of Pittsburgh, with John T. Flanagan, professor of psychology at Pittsburgh, as principal investigator, and John T. Dailey, research associate, as program director.

#### Stanford Conference

Still another effort to improve high-school education in the United States occurred 24–27 January, when 15 scholars, educators, and lay authorities gathered at Stanford University's Center for Advanced Study in the Behavioral Sciences for exploratory talks on how to strengthen the national public-school curriculum. The group—which included Conant and Graham DuShane, editor of *Science*—was under the cochairmanship of Ralph Tyler, director of the study center, and Paul R. Hanna, professor of child education at Stanford. The conference was supported by the Ford Foundation's Fund for the Advancement of Education. Three questions were considered.

1) How can we achieve a solution to the curriculum problem as viewed by the public and its legislators, by the scholars and scientists, and by the professional educators?

2) How can public-school curriculums adequately represent the national interest and at the same time the special needs of the local and state community?

3) What steps can be taken and what procedures should be followed toward development of a more adequate public-school curriculum?

#### Hutchins' Comments

In contrast to Conant, Robert M. Hutchins, president of the Fund for the Republic, had some sharp words for American education when he received the Hillman Foundation Award for meritorious public services on 22 January. He said that every citizen must be educated to the limit of his capacity if this nation is to survive as a democracy in the nuclear age, that to preserve the democratic faith men must be sufficiently informed to take part in making decisions in a free society: "I don't mean trained, amused, exercised, accommodated or adjusted; I mean that his intellectual power must be developed."

He then went on to say that history would have trouble assessing American education in the 20th century.

"It will see a people, who say they are dedicated to education and who are the richest in the world, indifferent to education and unwilling to pay for it. It will see an educational system that delivers less education per dollar than almost any other saying that all it needs is more money. . . . History will smile sardonically at the spectacle of this great country's getting interested, slightly and temporarily, in education only because of the technical achievements of Russia, and then being able to act as a nation only by assimilating education to the 'cold war' and calling an educational bill a defense act."

#### Humphrey Explains Detection of Nuclear Weapon Testing

In a speech given 20 January to his colleagues, Senator Hubert H. Humphrey, Democrat from Minnesota, gave the sources of the new scientific data on nuclear test detection and related these data to the talks now in progress at Geneva. These negotiations stem in part from an agreement reached last summer by Soviet and Western scientists that a detection system was feasible. A White House announcement early last month cast some doubt on the bases for that agreement.

The senator, chairman of the Foreign Relations Subcommittee on Disarmament, served as congressional adviser to

the United States delegation at these talks prior to the opening of the new Congress. Excerpts from his speech follow.

"The Geneva negotiations have been making slow but, nevertheless, steady progress. Although it is still unclear as to whether the negotiations will succeed, and most of the difficult issues must still be discussed, agreement has been reached on four articles of a treaty."

#### Exaggerated Claims

"In the midst of these negotiations the White House issued a statement, on January 5, 1959, to the effect that new scientific data have appeared which indicate that 'it is more difficult to identify underground explosions than had previously been believed.' The White House announcement, which was made by the President's Science Advisory Committee, has resulted in some exaggerated statements and claims. On the one hand, some people charge that the new information is not genuine and that it has been put forth by those who do not want to see a ban on tests. One correspondent, for example, says, 'There is something fishy about the timing and the content of the White House announcement. Is this another underground triumph for Dr. Teller? . . . Are they (the scientists) jumping to conclusions they have long sought in order to block a test cessation they have always opposed?'"

"On the other hand, there are claims that the new data make an agreement to ban tests under effective control impossible, and we should immediately call off the negotiations. To cite a case in point, a prominent magazine writes that the announcement means that 'the real minimum underground blast that could be fully detected was about 20 kilotons—about the size of the Nagasaki-Hiroshima bombs. . . . Should the U.S. trust to any stop-test agreement where the chances of deception are so great as to be a major risk of survival?'"

#### Data Are Not False

"The White House announcement is not the result of data trumped up by Dr. Teller or anyone else. It is true that Dr. Teller does not favor a ban on nuclear weapons tests but insofar as I am aware Dr. Teller had nothing to do with the gathering of the new material. He may be using it to try to persuade people that the test ban negotiations should be called off but he did not create the data."

"How, then, did the data originate?"

"When the President, on August 22, 1958, indicated that the United States would stop testing October 31, 1958, pending the outcome of the political negotiations of a test ban treaty, the Atomic Energy Commission quickly planned several tests at the Nevada proving grounds during September and October. Four of these were underground

tests. Two were below one kiloton, one was approximately 4.5 kilotons, and one was about 23 kilotons."

"During the month of November seismologists reported to the U.S. Government as to whether and how their seismographs picked up the four tests. This material was received by two groups. One was the U.S. Coast and Geodetic Survey and the other was the group responsible for detecting nuclear tests."

"The records of the seismologists were analyzed further by a special panel of scientists from various governmental agencies and from private institutions during the test ban conference recess, from December 16 to January 5. Although the study of what should and could be done to improve the control system had not been completed, the Science Advisory Committee decided to issue a general statement to the public. At the same time the United States made the data and an analysis of it available to the Soviet Union and the United Kingdom, the two other nations participating in the test ban negotiations. On January 16 the Defense Department released more detailed information to the press."

"I cite these events to show that the data presented are genuine and not something that has been invented by those who wish to scuttle the test ban negotiations. However, this does not mean that the data prove that detection of nuclear tests has now become so difficult that, to quote again from a prominent periodical, 'the chances of deception are so great as to be a major risk to survival' because 'the real minimum underground blast that could be fully detected was about 20 kilotons.' This conclusion not only is invalid. It is factually incorrect and completely misleading."

#### What the Data Showed

"The analysis by the panel of scientists seemed to indicate that the control system as devised by the Geneva Conference of Experts, and which was based on scientific evidence available at the time, would have a more difficult job to distinguish between nuclear explosions and earthquakes than had previously been realized. The new data also showed that 'seismic signals produced by explosions are smaller than had been anticipated and that there are consequently about twice as many natural earthquakes equivalent to an underground explosion of a given yield as had been estimated by the Geneva Conference of Experts.'"

"It is important to stress that the new information does not bear so much on the detection of nuclear tests as it bears on the identification of earthquakes which must be made so that they can be distinguished from nuclear explosions and thus eliminated by the control organization as suspicious events. Signals

received at control posts which cannot clearly be identified as earthquakes must be subject to on the spot inspection, according to the Geneva technical agreement on detecting tests."

#### Improvements Possible

"The significance of the new data and what can be done about it are subject to varying interpretations. Some view the information in such a pessimistic light that they advocate suspending only atmospheric tests but continuing underground tests. Others are more optimistic and believe that improvements can be made so that the risk to the United States of entering into a test ban agreement will not be significantly increased. In fact, the new data on underground explosions prompted further research and study of detection methods. Already new methods have been found."

"I understand that there are several promising techniques to improve the capabilities without increasing the size of the system recommended at Geneva. These include: building better instruments, devising ways to blot out background noise, placing seismographs deep in the earth, substantially increasing the number of seismographs at each control post over the number provided for in the Geneva report, utilization of additional methods to distinguish earthquake signals from nuclear explosions other than the determination of first motion, and use of unmanned seismographs to augment the Geneva system. Another obvious improvement is to increase the number of manned control posts. The President's Science Advisory Committee is continuing to study how the control system might be improved. I hope and urge that its report be made public."

#### Basis of the Geneva Conclusions

"To view the new data in perspective, it is worthwhile to recall the basis of the conclusions of the Geneva Conference of Experts regarding the detection of underground nuclear tests. The Geneva conclusions are based on the following premises: that if five or more seismographic stations at various directions from the source of an underground event are able, not only to detect the event, but to determine the direction of the first motion on the seismogram—that is, whether the first motion is compressional (line on seismogram goes upward) or dilatational (downward first motion on seismogram—that then some 90 percent of the earthquakes can be eliminated as not being nuclear explosions or suspicious events. Whether the five stations can determine first motion depends somewhat on the distance of the seismograph from the source of the event (at certain distances the direction of first motion is difficult to determine) and on the amplitude, i.e. strength, of the signal. Deter-



mination of first motion also depends on whether the seismographic station is at a site which is relatively free of noise from other factors. If the size of the wiggle on the seismogram showing the direction of the first motion is quite small, and if there is substantial background noise at the seismographic station, then the direction of the first motion might be extremely difficult to determine correctly. Signals that cannot be definitely determined as coming from earthquakes, according to the Geneva conclusions, must be subject to on the spot inspection.

"The scientists at Geneva also thought that if five seismographic stations recorded some compressional waves as first motions and some dilatational or rarefaction first motions that this was a definite sign of an earthquake, and, therefore, probably need not be investigated. However, if all or most of the seismographic stations recorded the direction of the first motion as compressional then this was to be regarded as suspicious evidence that a nuclear test has occurred. An investigation would then be permissible. According to the report of the panel of scientists who studied the results of the recent underground tests, the new data do not change this conclusion.

"The number of stations recommended by the Geneva Conference of Experts was based on identifying about 90 percent of the earthquakes equivalent to a nuclear explosion of five kilotons and thus eliminating them as suspicious events. The capability of the control system devised at Geneva to identify underground events of less than five kilotons would depend on: 'a) the small fraction of earthquakes that can be identified on the basis of data obtained from the posts alone; b) the fraction of earthquakes that can be identified with the aid of supplementary data obtained from existing seismic stations; and c) the fraction of events still left unidentified which could be suspected of being nuclear explosions and for which the international control organ carries out inspection. . .'

"The control organization would also have a capability to deter a potential violator so long as there existed the right of spot checking the source of signals which appeared suspicious. In other words, the right of inspection would exist even though it would not be practical to investigate the source of every signal that could not be identified as coming from an earthquake."

#### Number of Earthquakes Still Uncertain

"In deciding the number of earthquakes that would need to be identified and, therefore, eliminated from inspection control the Conference of Experts was somewhat vague. The reason for this vagueness in part stems from a difference of view between the Western and Soviet scientists as to how many earthquakes of

a given size occur each year. The difference of opinion over this matter appears to be not unusual. Seismologists and geologists have not heretofore been concerned with small earthquakes and have, therefore, not concentrated on devising seismographs or seismographic sites to record them. They have been primarily concerned with the larger earthquakes which could be a danger to cities and populations.

"The number of earthquakes that occur each year of various sizes, therefore, is at present a mathematical calculation or an extrapolation downward of what has been known about the number of large earthquakes. The number of earthquakes is not yet determined as a result of close and persistent scientific observation. For this reason, a great deal more research must be done before scientists have reliable knowledge in this field.

"The new data presented to the President's Science Advisory Committee indicate that since an underground test might give off a weaker signal than previously believed by Western and Soviet scientists, that therefore, the control system will have more earthquake signals to cope with and thus have more of a burden placed upon it than had previously been thought. And, to repeat, the fact that some nuclear explosions give off weaker signals makes the determination of the direction of the first motion more difficult. But again to repeat, many of the scientists who have studied these results feel that through improvements in scientific instruments and further research, the capabilities of the control system can be increased. In fact, the President's Science Advisory Committee has so indicated."

#### Implications

"The new data do not indicate that nuclear tests will definitely be more difficult to detect. Actually, the data appear to indicate that in some respects it may be easier to detect nuclear tests than the Conference of Experts at Geneva had concluded. This is so because the Geneva conference thought that the determination of the direction of the first motion was about the only way that earthquakes could be distinguished from nuclear explosions. The new data appear to show that surface waves from a nuclear explosion are weaker than those from earthquakes of a comparable size and that, therefore, analysis of surface waves may be used to distinguish between the two.

"In addition the Geneva Conference thought that at certain distances, from about 620 miles to 1240 miles from the source of an event, seismic signals would be very weak. The new data indicate that stations in this 'shadow zone' as it is called pick up signals somewhat stronger than had been estimated. The

size of the zone is now considered somewhat larger; the signals are delayed, and determination of first motion is not usually considered possible, but the strength of the signals is greater."

#### Rockefeller Institute Press

The Rockefeller Institute and the Oxford University Press in New York have jointly announced the establishment of the Rockefeller Institute Press to publish books on science and related subjects. Editorial responsibility for the books will rest primarily with the institute, while Oxford will be chiefly responsible for design, printing, and distribution. It is expected that the first books will be issued in the fall of 1959. While many of the books and monographs will be the work of institute faculty members, works by other scholars will also be published.

In announcing the new press, Detlev W. Bronk, president of the institute, said:

"The functions of a university include not only the advancement of knowledge and its communication to a new generation of scholars but also the dissemination of new knowledge far and wide. It is appropriate, therefore, that The Rockefeller Institute, having become a graduate university of science in 1954, should now follow the example of its sister institutions throughout the world in establishing a university press.

"Among the objectives of the press will be to provide an additional and needed outlet for scientific books of highest quality, carefully selected for their excellence. Further, during this time of rapidly increasing scientific knowledge paralleled by ever-mounting costs of publication, The Rockefeller Institute Press, because of its nonprofit nature, will make possible the publication of books at costs which will enable the



Charles I. Campbell

individual to purchase them for his personal library."

The Rockefeller Institute Press will be under the direction of Charles I. Campbell, administrative associate for information services at the institute. Production and distribution of the institute's journals will continue to be managed by Florence M. Stewart as head of the journals' department of the press. The institute publishes the *Journal of Experimental Medicine*, the *Journal of General Physiology*, and the *Journal of Biophysical and Biochemical Cytology*.

### Overseas Agricultural Research

Foreign scientists will work in the interest of American farmers, as well as for the agriculture of their own countries, through a recently announced program of foreign research directed by the U.S. Department of Agriculture. An article in the current issue of the department's publication *Foreign Agriculture* states that benefits from the new program may include expanded markets for U.S. farm output, new uses of agricultural products, and the development of new crops.

Payment for the research will be made in the form of both grants and contracts. The money will come out of foreign currencies accruing from the sale of surplus farm commodities under the Agricultural Trade Development and Assistance Act of 1954. The research will be done at foreign scientific institutions in four general fields—marketing, utilization of farm products, farm production, and forestry.

### Canadian Aeronautical Establishment

The Canadian National Research Council has announced the creation of a new division to be known as the National Aeronautical Establishment. The formation of the National Aeronautical Establishment, which consists of the aeronautical research facilities of the NRC Division of Mechanical Engineering, was authorized initially in 1951. The scale of operations has reached a point at which it becomes more practicable administratively to separate the Establishment from the Division of Mechanical Engineering and form a new division. The resources of the new division will consist of the Flight Research Hangar and Laboratories now at Uplands Airport, the new 5-foot supersonic wind tunnel now being constructed at Uplands Airport, and the Aerodynamics and Structures Laboratories in the NRC's Montreal Road Laboratories.

The Establishment will work closely with the Canadian Defence Research Board on defense problems in aeronautical science, and will also be responsible

for meeting the aerodynamic and structural research needs of civil aviation and of the aircraft industry. Acting director of the new unit is Frank R. Thurston, head of the Structures Laboratory of the Division of Mechanical Engineering.

### Baby Tooth Survey

Plans to collect 50,000 baby teeth a year to provide a record of the absorption of strontium-90 by children in the St. Louis, Mo., area have been announced by the Greater St. Louis Citizens' Committee for Nuclear Information. Parents and children in the St. Louis area are being asked to participate in this project by mailing deciduous teeth to the Baby Tooth Survey. Public interest in the strontium-90 problem in the St. Louis area has been stimulated by reports that among five areas tested, milk from the St. Louis milkshed contains the highest strontium-90 levels for 1958.

In announcing the survey, the committee cited an article that appeared in *Nature* [182, 283 (2 Aug. 1958)] by Herman M. Kalckar, a biochemist at Johns Hopkins University, calling for the establishment of a program of tooth collection for strontium-90 analysis throughout the world. In this article Kalckar states, "Such an International Milk Teeth Radiation Census would contribute important information concerning the amount and kind of radiation received by the most sensitive section of any population, namely, the children. At present important although rather erratic data exist, based on autopsy of bone samples derived mainly from adults."

In establishing the Baby Tooth Survey, the committee said that, as far as it knows, it is the first group to initiate a large-scale collection of deciduous teeth.

### International Federation of Operational Research Societies

The International Federation of Operational Research Societies came into existence on 1 January. Its objects are "the development of operational research as a unified science and its advancement in all nations of the world." The initial membership of IFORS consists of the Operations Research Society of America, the Operational Research Society (United Kingdom), and the Société Française de Recherche Opérationnelle. Membership is open to other national societies whose primary object is the advancement of operational research and whose membership includes qualified scientists working in this field.

The federation will be governed by a

board of representatives, one representative coming from each member society. According to the statutes, the voting power of each representative is proportional to the square root of the size of the membership, a formula designed to give the right weight to size.

One of the first activities of IFORS will be to sponsor the second international conference on this subject, following the successful first conference held in Oxford in 1957. The second conference is provisionally planned to take place at Aix-en-Provence in southern France in early September 1960.

Sir Charles Goodeve has agreed to act as the first secretary of IFORS, and Donald Hicks as treasurer. The address of the new federation is 11 Park Lane, London W.1, England.

### German Physicists Oppose Atom Weapon Research

The 3000-member Union of German Societies for Physics met on 5 October in Essen, West Germany, and issued a statement condemning the nuclear arms race. The text of the statement and a report of the meeting as it appeared in the October newsletter of the Society for Social Responsibility in Science, follows:

"German physicists are deeply concerned at the increase of nuclear armaments everywhere. The Union of German Societies of Physics therefore once more warns the public that the use of these weapons in war will inevitably lead to the annihilation of millions of people and to complete devastation through radioactivity.

"The physicists, who desire their work to benefit mankind, repeat their previous warning as to the consequences which a criminal misuse of the results of their research might have. They wish to state with all possible emphasis that nuclear weapons are capable of the wholesale destruction of all races and will expose to the horrors of death by radiation even those nations which are not involved in the conflict.

"On behalf of its 3000 members, the Union of German Societies of Physics again urgently appeals to the public, and in particular to responsible politicians in all governments and parliaments, to give unceasing and constant support to any attempts at a peaceful settlement between the States and at last bring to an end the atomic arms race, including nuclear tests."

The meeting unanimously agreed to appoint a special committee to look after the interests of those scientists who "suffer disadvantages" as a result of adhering to the pledge in clause 2 of the Union's constitution "to bear in mind that those who work in sciences are re-

sponsible to a particularly high degree for the shaping of human lives."

Members were reminded that "the progressive development of nuclear weapons and other means of mass destruction of every kind is generally impossible without the active cooperation of physicists."

The terms of reference of the committee are "to instruct the public, as well as all physicists, of the dangers of nuclear weapons and all other means of mass destruction; to immediately safeguard the interests of all members affiliated to the Union in regard to the decision of them; to advise and support members of any situation of conflict which may arise from an adherence to their duties as laid down in Clause Two of the Constitution.

### News Briefs

The British Medical Association's Science Committee has asked 60,000 physicians for confidential reports on the case histories of babies conceived through artificial insemination. The committee plans to use the data from the reports as evidence before a government inquiry into artificial insemination. The inquiry will examine the legal and moral issues as well as medical aspects of artificial insemination.

\* \* \*

The Special Training Division of the Oak Ridge Institute of Nuclear Studies has announced a series of courses to be held in Oak Ridge, Tenn., this year. The 1959 curriculum includes courses in the basic techniques of using radioisotopes, courses in the industrial uses of isotopes, and a special summer institute for secondary-school science teachers. A maximum of 48 participants will be accepted in basic courses and in the summer institute. Industrial courses will be limited to 32.

\* \* \*

The University of Chicago has announced the establishment of the Comdr. Eugene F. McDonald, Jr., Memorial Laboratory for Exfoliative Cytology at the University of Chicago Medical Center. Formation of the laboratory was financed by leaders in the radio and television industry as a memorial to the founder-president of Zenith Radio Corporation of Chicago. He died in May 1958.

\* \* \*

The Atomic Energy Commission has established a training program at the Shippingport Atomic Power Station for supervisory personnel of domestic and foreign organizations engaged in or planning the design, construction, or operation of nuclear power plants. The training program, which will be conducted

by the Duquesne Light Company for the commission, will enroll 25 participants every 3 months for a 6-month course.

### Grants, Fellowships, and Awards

**Meteorology.** The Alfred P. Sloan Foundation has granted \$45,000 to the University Committee on Atmospheric Research to encourage graduate study of meteorology and closely allied sciences. Ten fellowships for the academic year 1959-60 will be awarded. Recipients will each receive \$4000 and may attend any qualified institution offering a graduate program in physical sciences of the atmosphere.

Universities represented on the Committee on Atmospheric Research are the University of Arizona, University of California, University of Chicago, Cornell University, Florida State University, Johns Hopkins University, Massachusetts Institute of Technology, University of Michigan, New York University, Pennsylvania State University, St. Louis University, A. & M. College of Texas, University of Washington, and University of Wisconsin.

Applications must be filed by 28 February and the awards will be made on 31 March. Application forms may be obtained from the University Committee on Atmospheric Research, P. O. Box 3297, MSS, Tallahassee, Fla.

**Microbiology.** The Foundation for Microbiology, established in 1951 by Selman A. Waksman, is prepared to receive applications for grants in various fields of microbiology. The trustees of the foundation have decided to give first consideration to applications dealing with problems of publication of monographs or large papers on microbiological subjects for which it is difficult to obtain financial support otherwise. Applications should be addressed to the Foundation for Microbiology, c/o Institute of Microbiology, Rutgers State University, New Brunswick, N.J.

**Neurophysiology.** The Marine Biological Laboratory at Woods Hole, Mass., is offering a training program in neurophysiology under the direction of S. W. Kuffler, C. L. Prosser, and E. J. Furshpan. Financial aid will be provided to nine pre- and postdoctoral fellows between 20 June and 31 August. There will be opportunities for participating in current research programs at the laboratories and for learning techniques and theory of neurophysiology, but no formal lecture course is planned. Applications should be made to the Director, Marine Biological Laboratory not later than 15 February.

**Ornithology.** Awards for ornithological research are made in April of each year by the Frank M. Chapman Memo-

rial Fund Committee of the American Museum of Natural History, New York. Applications should be received by 15 March 1959. For information write to: Wesley E. Lanyon, American Museum of Natural History, Central Park West at 79 St., New York 24, N.Y.

**Physiological Psychology.** The College Council of St. John's College, Cambridge (England), proposes not later than the end of April to make an election to the Kenneth Craik Research Award for the assistance of persons engaged in postgraduate research, preferably in physiological psychology. Persons of either sex and of any academic standing are eligible. The person to whom the award is made need not be and shall not be required to become a member of the college and need not reside in the University of Cambridge, but shall submit reports on the place, nature, and progress of his or her research as the council may require. The award shall be tenable, subject to the consent of the council, with any other emolument and with a salaried post.

The value of the award will be £450 a year. The council may pay to the holder such part of the award as it shall decide in the form of a grant or grants to meet specified costs to be incurred in connection with the research undertaken, which costs may include the costs of travel, of the purchase or hire of apparatus or other equipment, or of the provision of technical or clerical assistance.

The award will be tenable from 1 October 1959 for such period, not less than 1 year nor more than 3 years, as the council shall decide, and if the election is made for a period of less than 3 years the tenure may at the discretion of the council be prolonged for a further period or periods, provided that the award shall not be held by the same person for more than 3 years in all.

Applications should be sent to The Master, St. John's College, Cambridge, so as to reach him not later than 14 March, accompanied by full particulars of the applicant, a statement of the nature and probable duration of the postgraduate research contemplated and of the place where it is intended to pursue it, particulars of any further assistance the applicant expects to receive, and the names and addresses of not more than three persons to whom the council, if they wish, may refer. Testimonials should not be sent.

### Scientists in the News

President Eisenhower has appointed DON K. PRICE, JR., new member of the AAAS Board of Directors, to his Advisory Committee on Government Organization. Price, who is dean of the



Harvard Graduate School of Public Administration, succeeds Nelson A. Rockefeller, the new governor of New York. Rockefeller served as chairman of the committee, a position that has now been assigned to ARTHUR S. FLEMMING, Secretary of Health, Education and Welfare.

ABRAHAM HORWITZ of Chile assumed office as director of the Pan American Sanitary Bureau on 1 February. He succeeded FRED L. SOPER of the United States, who became director emeritus after having completed three 4-year terms as bureau director. At the time of his election to the PASB, Horwitz was assistant director of his country's National Health Service and professor of preventive medicine at Chile's National University.

The first annual Louis W. Hill Space Transportation Award has been made posthumously to the distinguished American rocket pioneer, ROBERT H. GODDARD (1882-1945). The award, which carries an honorarium of \$5000, was presented to the scientist's widow at the annual Honors Night Dinner of the Institute of the Aeronautical Sciences, which took place on 27 January in New York.

The new award was established by the Louis W. and Maud Hill Family Foundation of Minneapolis to honor American scientists for "research in any or all of the fundamental sciences relating to space travel or space technology" and is administered by the IAS. Although it was established to encourage and reward living scientists, both the Award Board and the Hill Foundation agreed that the first recipient should be Dr. Goddard, who, despite opposition and ridicule during more than 40 years of determined effort, tremendously advanced the science of rocketry.

Goddard began his rocket researches in 1899 when he was 17 years old. Many of the techniques which he first developed and tried out are now regarded as basic. He first suggested the multistage rocket in 1909. Other modern devices which resulted directly from his work are the Army's "bazooka" launcher and JATO (jet assisted take-off), now widely used by many types of military aircraft.

Although the importance of his work was frequently not appreciated during his lifetime, Goddard was materially assisted by a grant from Harry Guggenheim and, during his later years, by the U.S. Navy.

At the recent joint annual meeting of the American Physical Society and the American Association of Physics Teachers, PAUL KIRKPATRICK of Stanford University received the Oer-

sted Medal, CHARLES H. TOWNES of Columbia University delivered the 17th Richtmyer Memorial Lecture of the AAPT, CONYERS HERRING of Bell Telephone Laboratories received the Oliver E. Buckley Solid-State Physics Prize, MRS. LLOYD W. TAYLOR received a scroll as part of the Taylor Manual Ceremony, and JAMES G. POTTER of the Agricultural and Mechanical College of Texas and WILLIAM C. KELLY of the American Institute of Physics received AAPT Distinguished Service Citations.

JOHN B. NELSON of the Rockefeller Institute for Medical Research has received the Charles A. Griffin Award of the Animal Care Panel in recognition of his work on the control of diseases of laboratory animals. Nelson's research on endemic pneumonia of rats has led not only to control of this respiratory infection but also to elimination of many other diseases from animal colonies.

L. N. H. BUNT, professor of mathematics at the University of Utrecht, Netherlands, is at present a visiting professor at Teachers College, Columbia University. Bunt is a member of the Subcommittee for the Netherlands of the International Commission on Mathematical Instruction. Recently he served as adviser to the Minister of Education of the Government of Brazil for a program on the teaching of mathematics in secondary schools and colleges.

V. A. ENGELHARDT, director of the Biochemical Institute, Moscow, recently spent 2 days lecturing and visiting the Radiation Laboratory and the Donner Laboratory at the University of California, Berkeley. He was accompanied by Mrs. Engelhardt, a biochemist with whom he has collaborated in studies on the chemistry of muscular contraction.

WILLIAM B. BEAN, professor and head of the department of internal medicine at the State University of Iowa, has been elected a fellow of the Royal Society of Medicine, London.

JOHN C. RIEDEL has been appointed senior project engineer, electronics, for the Endevco Corporation, Pasadena, Calif., an electronic instrumentation firm. Formerly he was test engineer with Consolidated Electrodynamics and supervising engineer of the electronics group at Northam Electronics.

Also at Endevco, DONALD E. LOVELACE, previously in the Transducers Division of Consolidated Electrodynamics, has been appointed senior project engineer, transducers.

## Recent Deaths

WILLIAM S. BRACKETT, Charleston, W. Va.; 60; since 1944, vice president in charge of engineering of the Union Carbide Chemicals Company; 8 Jan.

RUSSELL S. FOWLER, New York, N.Y.; 84; honorary director emeritus of surgery and chief surgeon for 43 years of the Wyckoff Heights Hospital, Brooklyn; one of the founders of the American College of Surgeons, and founder and past president of the Clinical Society of the Wyckoff Heights Hospital; co-developer of Fowler's position, a resting position for postoperative patients; author of several books on surgery; 7 Jan.

ERNST K. JORDAN, London, England; 97; entomologist who went to England from Germany in 1893 to serve as curator of entomology at the Zoological Museum at Tring, Hertfordshire, England; in 1930, named director of the museum, his post at retirement in 1939; internationally known for his studies of parasitic insects; 13 Jan.

SIDNEY I. KORNHAUSER, Louisville, Ky.; 71; professor and chairman of the department of anatomy at the University of Louisville School of Medicine, 1922-58; chairman of Commission on Biological Stains; 1 Jan.

RAYMOND R. LANIER, Denver, Colo.; 44; professor and head of the department of radiology at the University of Colorado since 1950; taught gross anatomy at Washington University, 1937-42, and roentgenology at the University of Chicago, 1948-50; was the central figure in a national controversy three years ago when he warned that radioactive dust from the Nevada atomic tests had become a menace to public health; 24 Nov.

AUGUST M. MEULEMANS, Kansas City, Mo.; 69; head of the department of biology, Rockhurst College, for 27 years; taught at the University of Wisconsin and at Xavier College before joining Rockhurst; 22 Dec.

SAMUEL PENNELL, New York, N.Y.; 58; associate attending physician and hematologist at the Maimonides Hospital; invented an apparatus for performing direct transfusions; 6 Jan.

MARY M. ROBERTS, New York, N.Y.; 82; editor-in-chief of the *American Journal of Nursing* from 1921 until her retirement in 1949; author of several books on nursing; 9 Jan.

*Erratum:* The levels enzyme activity given in Table 1 of the report, "Red Cell Glucose-6-Phosphate and 6-Phosphogluconic Dehydrogenases and Nucleoside Phosphorylase," by P. A. Marks [*Science* 127, 1338 (1958)] are incorrect by a factor of  $10^3$ . All activity levels should read per  $10^9$ , not  $10^6$ , erythrocytes.

## Book Reviews

**An Anthropologist at Work.** Writings of Ruth Benedict. Margaret Mead. Houghton Mifflin, Boston, 1959. xxii + 583 pp. \$6.

The subtitle states that this book is about the work and writings of Ruth Benedict (1887-1948), whom the author regards as a "figure in transition." By *transition*, Dr. Mead seems to mean that Ruth Benedict was the intellectual link between Franz Boas, her teacher and later her colleague, and the present-day studies of national character which are so well represented by Dr. Mead and her associates. I am sure, however, that any reader will be far more interested in the insights this book affords into the always-fascinating question of how a renowned scholar happened to enter his particular profession and make his special contributions to it than in any finely reasoned analysis of his role in the history of science.

As a scholarly profession, anthropology has drawn more than its share of non-conformists who are comforted by its findings that each culture has its own values and standards of behavior and that the demands of our own society are no more right in an absolute sense than those of any other. Ruth Benedict, however, seemed to be an exception. Her outward calm, mild demeanor, and Mona Lisa smile seemed to indicate a good adjustment to her world. The error of this inference is startlingly disclosed by the materials published for the first time in Dr. Mead's book. These materials reveal a tortured, nonconformist individual who finally found a creative outlet, and we hope relief, in anthropology. From this point of view the book might well have been entitled "The Making of an Anthropologist."

Dr. Mead does not present the life of Ruth Benedict as an ordered, direct, biographical narrative. Instead, she has assembled what might be called case history materials which can be read in any order. While a few poems, essays, and key scientific articles are republished, the more interesting materials consist of unpublished essays, poems, fragments of diaries, an intended book on Mary Wollstonecraft, and correspondence between Ruth Benedict and Edward Sapir, Franz Boas, and Margaret Mead. Margaret Mead, who was Ruth Benedict's intimate from 1922 until her

death, inevitably looms so important that the book is partly autobiographical.

Ruth Benedict's diaries and an unfinished autobiographical sketch, "The Story of My Life . . ." (pages 97-112), expose with surprising candor the black depressions and self-doubts that made her early life almost insupportable. These feelings, however, were so carefully concealed that Ruth Benedict was in effect two persons, a private self and a social self. The double pattern began in her earliest childhood, when Ruth Benedict shut part of herself off from her friends and family and lived in a secret world of imagination. This imagination later found expression in poetry which, however, was written under a *nom de plume*. It was not until she was well established in anthropology that Ruth Benedict's somewhat secret literary endeavors were abandoned and her private and public selves merged in creative professional work.

The inner torments and introspective search for an answer to life continued well into adulthood. Ruth Benedict tried teaching and embraced social causes to no avail. In large measure, she saw her difficulty as a consequence of being a woman in our own culture, and she worked on biographies of famous emancipated women. The only one of these published is that of Mary Wollstonecraft which appears for the first time in the present book. "So much of the trouble," she wrote in 1912, "is because I am a woman. To me it seems a very terrible thing to be a woman." Her final success consisted partly in overcoming this handicap. But in 1913 she said, "There is one crown which perhaps is worth it all—a great love, a quiet home, and children" (page 20), and "My jobs have never been an end in themselves, always just a means and a subordinate means" (page 128).

This hope of release seemed realized in 1914 when she married Stanley Benedict, a biochemist, and wrote, "I have attained to the zest for life . . . I have looked in the face of God. . . ." (page 129). But the relief was transient, the fits of doubt and depression recurred, the need for self-expression continued, she had no children, and she and her husband separated in 1930.

Resolution of her difficulties did not immediately follow her introduction to anthropology. She first took anthropology

courses in 1919, and in 1922 at the age of thirty-three she took a Ph.D. in anthropology under Franz Boas at Columbia University, where she remained most of her subsequent years. But for some time during the 1920's, Ruth Benedict continued to express herself in poetry. Some of the most fascinating portions of the book contain letters between herself, Edward Sapir, and Margaret Mead, all poetically minded, who discuss their poems and their largely fruitless efforts to publish them.

Gradually, Ruth Benedict devoted her efforts more exclusively to anthropology, in which she found a peculiarly appropriate answer to her personal needs. Boas had been interested in the diffusion of the elements or ingredients of cultures and their integration in the lifeways of particular societies. Benedict was less interested in diffusion or history than in how each culture synthesized these elements and stamped them with over-all patterns that emphasized special values within the total range of possibilities. Thus her personal unorthodoxy found free play in reviewing the many possible patterns in which one or another kind of personality might feel at home.

Benedict's outstanding contribution to anthropology was to stress the distinctive pattern of each culture, and she did this in literary or value terms. In *Patterns of Culture* (1934), which first expressed her views most fully and brought her international fame, she adopted Nietzsche's characterizations, "Apollonian" and "Dionysian," to contrast the frenzied behavior patterns of the Plains Indians with the serenity of Pueblo life. *The Chrysanthemum and the Sword*, an amazingly incisive and penetrating analysis of Japanese behavior (written in Washington during World War II and published in 1946) still deals essentially with adult behavior patterns and pays comparatively little attention to the psychoanalytic techniques which had become fashionable by that time.

To outward appearances, success as a woman and as a scholar had resolved Ruth Benedict's inner conflicts during her later years. In 1926 she had written (page 153), "I know to the bottom of my subconsciousness that no combinations of circumstances, no love, no well-being will ever give me what I want. But death will." The full measure of her success came late, just before her death in 1948: the presidency of the American Anthropological Association, the first full professorship held by a woman at Columbia University which she had served so well and so long and the privilege of attending the hallowed and heretofore-all-male faculty meetings, the Annual Achievement Award of the American Association of University Women, and many other honors as well as a research grant from the Office of Naval

Research. The grant was unbelievably large compared with the "slim pickings" of earlier years.

Since the book deals to a large extent with the development of anthropology, wherein Dr. Mead sees Ruth Benedict as playing a transitional role between Franz Boas and the kind of national character studies being carried on by Dr. Mead, a few comments are in order. Ruth Benedict can properly be considered transitional to the approach which seeks to understand national character by paying attention to infant care and child training. It should be noted, however, that just before World War II, Ruth Benedict, Ralph Linton, Abraham Kardiner, a psychoanalyst, and others held a series of important symposia at Columbia University. This was the turning point in the development of what was called the "culture and personality" approach. It is surprising that Dr. Mead does not mention this.

It should be made clear that the transition to the culture and personality and national character approaches was but one of many transitions from the broad range of interests involved in Boas' work. Dr. Mead's statement (page 429) that when Ruth Benedict returned to Columbia University after the war she had to work "in isolation in a department which had been sedulously swept bare . . . of any signs of the Boas tradition" is both unkind and inaccurate. The appointment of Ralph Linton and W. D. Strong to the department just before the war and my appointment just after meant a diversification of the tradition, not a break with it. Dr. Mead herself says (page 345) of the so-called "Boas school" that "there was actually no such thing." Boas was the intellectual grandfather of most American anthropologists, and few advocates of any contemporary approach would presume exclusive rights to his mantle.

As a scientific exposition, Dr. Mead's book must be taken with the qualifications just suggested. As a fascinating source of insights into a remarkable woman presented by another remarkable woman, it will well reward any reader.

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**Solving the Scientist Shortage.** David C. Greenwood. Public Affairs Press, Washington, D.C., 1958. 69 pp. \$2.

Many speeches and reports that have been made over the past few years about the apparent shortage of scientists are summarized in this publication. After a sketch of the nature of the problem and the educational outlook, Greenwood turns to efforts (mostly proposals) from

governmental groups, private (mainly industrial) groups, and professional scientific and engineering groups. From an extensive bibliography he then selects a large number of recommendations for action. Probably his most significant suggestion is that for a single major organization to carry through various inquiries and to develop major and consistent lines of action.

Almost everyone has "gotten into the act" on manpower needs. A wide range of viewpoints and vested interests is represented by the proposals Greenwood reviews. To bring any order out of this mixture of special pleading, confusion, and contradiction would require many more than the 68 pages he has used. His eclectic approach, with brief descriptions of some industrial activities but without analysis of their significance, leads to citation of isolated authors and to contradictory proposals.

On the role of women in science and engineering, on page 13, he notes that in the U.S.S.R. women currently constitute 50 percent of all professionals. Then, on page 59, he cites a survey made in 1957 reporting that "only thirteen percent of the nation's college women are there primarily to receive an intellectual training" (one wonders what percentage is reported for the men!) and then proposes that all the 87 percent with "other primary purposes" be dropped out of college. Just how this is to be done, when, and by whom, and whether this would not cut even further into the potential womanpower pool, is not mentioned.

Greenwood's proposals range widely in diversity and difficulty of accomplishment. On page 52 (number 26 under "Industry") he states, "canteen meals in industrial plants should be scientifically planned to provide the maximum amount of energy-building nutrients." On page 57 (15 under "Government") he states, "The Defense Department would be reduced in size to a small policy-making and coordinating agency, as has been proposed independently by Donald Douglas, Sr., chairman of the board of Douglas Aircraft." Does he want to try to do this?

Just how all these "shoulds" are to be accomplished, by whom, and with what finances is never mentioned. Consider, for example, page 62 (3 under "Colleges and Universities"): "The number of engineering places available in the nation's colleges should be doubled immediately"—immediately no less!

In his comments on grade school and high school Greenwood cannot avoid poking at the so-called "progressive educationists," whatever that may mean. However, note the contradictions here: on page 60 (item 10) he says, "All steps should be taken to make the teaching of the technical subjects as inspiring as pos-

sible" (what does "inspiring" mean?), while on page 61 (item 10) he states, "Any steps which teachers can take to raise the academic tension in schools, and remove the 'Let's learn for fun attitude,' would be deeply appreciated by the majority of business and industrial leaders." Is he proposing that in school, in business, and in industry learning and creative work be made distasteful? Why do people do creative work anyway?

In short, Greenwood's approach is eclectic and uncritical; his book lacks synthesis, is contradictory, and is filled with impossible "shoulds."

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**The Black Fens.** A. K. Astbury. Golden Head Press, Cambridge, England, 1958 (distributed by W. Heffer, Cambridge, England). xi + 217 pp. Illus. 42 s.

Of all regional divisions on the palimpsest of Britain's cultural and physical geography, the Fens are the most distinctive. Formed from the lower flood plains of rivers draining to the Wash, on the east coast of England, the Fens are low, dead flat, and highly fertile and are kept free of water only by means of a complex artificial drainage system. The region has two distinct parts: silt Fens in the north, with essentially mineral soils, and black Fens in the south, with peat soils. A. K. Astbury's book *The Black Fens* represents yet another addition to a vast literature of British regional studies. Most have an almost purely local interest. Astbury's work, however, deserves wider attention, because of the unusual interest and agricultural importance of the black Fens.

*The Black Fens* is written in the didactic, slightly chaotic, British style typical of many such regional studies. Astbury addresses himself mainly to the reader with nonprofessional interests. Lack of bibliography or documentation reduces the volume's usefulness for American readers.

*The Black Fens* covers the formation, physical characteristics, hydrography, farming, settlement, transportation, and reclamation of the English peat Fens. Expressed thus, the coverage sounds fairly complete. However, the principal emphasis is on past and present waterways (perhaps not too surprising in a discussion of an area that would be largely submerged without artificial drainage). Much of this is rather tediously detailed for the casual reader; much of it also seemed rather speculative to me. Because of the author's focus of attention, little space is left for matters that do not have to do with running water. This is a pity,



for such important considerations as land use and settlement get relatively meager treatment.

However, the book has much charm and interest, even practical value for those interested in our own peat lands, such as the Everglades of Florida or the Sacramento-San Joaquin delta of California. Astbury has a fascinating theme—man's mighty struggle against water and the conversion of a marshy waste into the major tract of first-class arable land in the British Isles. Agriculturalists, reclaimers, geographers, and others with like interests will derive much instruction and diversion from this book.

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#### Soviet Research in Crystallography.

*Chemistry Collection No. 5*, vols. 1 and 2. English translation. Consultants Bureau, New York, 1958. 618 pp. vol. 1, \$30; vol. 2, \$100; set, \$115.

These two volumes contain selected papers from Russian journals translated into English, reproduced by photo-offset, and bound in paper. In spite of the title and supposed aim of this publication, it contains little of interest to the crystallographer. Volume 1 contains 60 papers in the general field of inorganic chemistry; volume 2, 33 papers dealing with a miscellaneous collection of topics, including x-ray spectrography, structure of glasses, and crystal growth.

It is of great interest to those of us who have no way to penetrate the language barrier to be able to read through these Russian papers in the way that we read through the Western journals in the library. It is a pleasure to discover papers such as that by D. A. Petrov and N. D. Nagoskaya on the phase diagram of the Al-Cu-Mg-Si system—a strikingly comprehensive and beautiful study of an exceedingly complex system. It is noticeable, however, that many techniques which are regarded as routine in this country are not made use of, apparently, in Russian laboratories: for example, x-ray methods are only rarely used in phase-diagram studies, and counter methods are not used at all in x-ray spectrography. One paper on heteropolymolybdate complexes displays a great confusion about the structural chemistry of these compounds—a confusion which is shared by most American chemists.

The main points of criticism of these volumes must be directed toward the editorial work, which leaves a great deal to be desired. The editors apparently have no concept at all of the meaning of the term *crystallography* to scientists, espe-

cially crystallographers. The bulk of volume 1 is devoted to phase-diagram studies of such systems as  $\text{CuSO}_4\text{—FeSO}_4\text{—H}_2\text{SO}_4\text{—H}_2\text{O}$ ;  $\text{LiCl—BeCl}_2\text{—H}_2\text{O}$ ;  $\text{H}_3\text{BO}_3\text{—KNO}_3\text{—H}_2\text{O}$ ;  $\text{KNO}_3\text{—KCl—KBr}$ ; and  $\text{K}_2\text{SO}_4\text{—K}_2\text{CrO}_4\text{—KNO}_3$ , most of which depend on classical thermal methods. None of these papers can in any sense be classified under crystallography. Volume 2 does contain some articles of crystallographic interest, such as papers on the structure of polyamides of dipheic acid (by S. S. Spassky and M. A. Mikhailova), optical properties and structure of polyiodides (by D. A. Godina and G. P. Faerman), crystalline modifications of plumbic fluoride (by Ya. Sauka), and oxonium ion in crystal lattices of inorganic compounds (by N. V. Shishkin) and a series of papers by V. Kurbatov on "The nature of crystals," which discuss binding energies in various types of crystals. This volume also contains a series of papers of particular (although not crystallographic) interest on the techniques of x-ray spectrography, by E. E. Vainshtein and his colleagues. There are no papers at all on crystal structure analysis in the modern sense.

Obviously, the editors intended to present in these books merely a sampling of papers from the Russian journals in the period 1949–1955. The merit of such a project might well be discussed, but even if it is assumed to be worth while, the result is spoiled by a complete lack of judgment in the selection of papers. During the period covered, scores of papers of great crystallographic interest appeared in the Russian journals. Why were the works of such eminent crystallographers as G. S. Zhdanov, N. V. Belov, and A. I. Kitaigorodskii completely ignored? Crystallographers would have welcomed complete translations of their works on such important crystal structures as heavy metal thiocyanate complexes, diopside, and epidote; on contributions to the theory of structure determination; and on many other topics well known to Western scientists through abstracts. Such glaring negligence could only be a result of failure to seek the advice of anyone connected with the field of crystallography.

The quality of the translations cannot be properly judged by one who is not familiar with the Russian language, but the general intelligibility of the texts appears to be fairly good, although occasional awkward passages and phrases are evident. The origin of the papers is identified only by a system of code numbers, which indicate the journal and year but not the page numbers. The code numbers refer to some master translation file which presumably is available to the reader through services supplied by the publishers. References given in the papers

themselves are, of course, translated in the normal manner. The quality of reproduction is fair, but in the copy examined there are several missing or blank pages. One paper is reproduced twice. The standards of quality do not seriously impair the usefulness of the material presented (except where a page is missing), but they fall somewhat short of those set by a similar project sponsored by the American Institute of Physics.

Strangely, there is no explanatory information anywhere in the two volumes concerning this ambitious translation project. No mention is made of any of the editors responsible for the work. It can only be said that the volumes are valuable in that they will make available in useful form in the libraries some parts of the Russian scientific literature, but such an investment for the personal library will generally be out of the question.

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#### Discussions on Child Development. A

consideration of the biological, psychological, and cultural approaches to the understanding of human development and behavior. *Proceedings of the World Health Organization Study Group on the Psychobiological Development of the Child*: vol. III, third meeting, Geneva, 1955. J. M. Tanner and Bärbel Inhelder, Eds. International Universities Press, New York, 1958. 223 pp. \$5.

This volume continues the *Discussions on Child Development* series, of which the earlier two volumes were reviewed in the *Scientific Monthly* [84, 323 (1957)]. The sessions focused on the development of sex differences and of individuality or ego identity. As a basis for discussion of the first topic there were presentations by Margaret Mead on the "Childhood genesis of sex differences in behavior" and by Erik Erikson on "Sex differences in the play construction of twelve-year-old children." To introduce the second topic, presentations were made by Erik Erikson on "The syndrome of identity diffusion in adolescents and young adults" and on "The psychosocial development of children." In addition to the members of the study group, D. Buckle, Julian S. Huxley, and Raymond de Saussure participated in the discussions. The volume is a well-edited condensation of a week's discussion that moves forward at a lively pace.

But because the discussion moves freely without close contact with data, the reader who seeks quantified and verified statements will be disappointed. Even in the presentation of the mate-

rial on sex differences in play construction, where condensation and presentation in statistical terms is clearly possible, what is presented is an account of individual cases illustrated by diagrams of individual constructions. How does one advance from a combination of anecdotes and observations on individual persons and individual primitive societies to generalizations that make possible the prediction and understanding of human behavior? Erik Erikson presents a diagram of various stages in the gradual unfolding of the human personality through psychosocial crises which is tied to broad age categories. When one examines the evidence presented in support of this summarization, one finds its basis to be psychoanalytic theory rather than an extensive series of empirically derived principles based on an adequate sampling of human beings at various developmental levels, with appropriate attention to statistical significance.

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**Economics of Mental Illness.** Joint Commission on Mental Illness and Health, Monograph Series, No. 2. Rashi Fein. Basic Books, New York, 1958. xx + 164 pp. \$3.

This volume, the second in a series of studies sponsored by the Joint Commission on Mental Illness and Health, defines and assesses the direct and indirect cost of mental illness.

The questions which the commission sought answers to include: How much does mental illness cost the people of the United States? How much would it cost to provide the highest possible standard of care for the mentally ill? Can we afford these costs? Could greatly increased expenditures be justified on economic grounds? Where is the money coming from?

Rashi Fein sorts out these questions into problems on which the economist can provide direction and those which lie outside his professional competence. "What society can spend (and ultimately what society should spend) depends on the value system that society holds to. It is obvious that society can spend much more on mental illness (or on anything) than it presently is doing. Whether or not it chooses to do so is another question. We can provide data to assist us in understanding the implications of additional expenditures, the economic benefits to be derived therefrom, the gains, the costs. These may aid in answering the question, 'What should society do?' They do not answer the question. The

answer is up to society. The question, 'What can society do?' cannot be answered." Given the costs of mental illness, especially the loss in earnings and production, the issue becomes more clearly: Can we afford to incur the costs of not spending?

Direct costs per annum, defined as the sum of public expenditures (national, state, and local) and of identifiable private expenditures for the care of the mentally ill, are estimated to exceed \$1.7 billion. This sum includes not only the purchase of goods and services but also cash payments to the disabled under the Veterans Administration program. Indirect costs per annum, defined as the loss in productive activity of persons resident in mental institutions and of those who because of mental illness are absent from work, are estimated to approach \$800 million. Estimates are also developed by means of other techniques of measuring indirect costs. These range upward to \$1.9 billion—the estimated present value of all future earnings of persons who represent first admissions to public prolonged-care hospitals in 1954.

The volume contributes importantly to the literature on the cost of illness and the price of health. It makes a substantial beginning toward the formulation of different concepts of economic loss (or indirect cost) from illness and the concepts appropriate to the different uses. The concept of annual production-and-earnings loss is distinguished from loss measured as the present value of future earnings. Gross-production loss is differentiated from a net concept in which a deduction is made for the costs of maintaining a life saved. The book refines some of the tools of measurement used in earlier studies. Work-force-participation rates rather than population, or labor force, aggregates are used to determine the man-years loss in production. Definitions are tied to those used for national income account estimates so that output loss may be related to national net income product.

I might mention some minor technical deficiencies, without intending to suggest that these detract from the general usefulness of the study. There is no indication that the author is familiar with several earlier studies on the costs of sickness, including the C.-E. A. Winslow volume prepared for the World Health Organization, the *Cost of Sickness and the Price of Health* (1951). The author does not appear to be familiar with the work that has been done on construction of work-life tables—a readily available tool which would have simplified some of the estimating and would, on the whole, have improved the author's product. A median wage-and-salary figure is used to convert work-force years to dollar earnings per annum, without an ex-

planation of the use of a median rather than a mean, or of why wages and salaries are applied in lieu of an earnings figure that includes self-employment earnings.

While there are several other minor technical deficiencies, Rashi Fein has performed well the task of pointing out the nature of the costs of mental illness, the effects of use of additional resources for the care and prevention of mental illness, and the economic costs of possible types of action—including, clearly, inaction as well. His work should prove useful as a guide to programing in the period ahead.

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**Handbuch der Physik.** vol. 45, *Nuclear Instrumentation II*. S. Flügge, Ed. Springer, Berlin, 1958. vii + 544 pp. Illus. DM. 128.

*Nuclear Instrumentation II*, volume 45 of the *Handbuch der Physik*, is the second of two volumes devoted to nuclear instrumentation. The first of these has not as yet been published. E. Creutz is coeditor of the instrumentation volumes, together with S. Flügge, who is responsible for the over-all editorial direction of this new edition of the *Handbuch*.

*Nuclear Instrumentation II* contains the following sections: "Ionization chambers in nuclear physics," by H. W. Fulbright; "Geiger counters," by S. A. Korff; "Scintillation and Cerenkov counters," by W. E. Mott and R. B. Sutton; "The proportional counter as detector and spectrometer," by S. C. Curran; "The coincidence method," by S. DeBenedetti and R. W. Findley; "Cloud chambers," by C. M. York; "The bubble chamber," by D. H. Glaser; "Nuclear emulsions," by M. M. Shapiro; "Detection of neutrons," by H. H. Barschall; and "High energy neutron detectors," by R. T. Siegel. All of the articles give a rather complete review of the literature up to about 1956-1957.

For such topics as ionization chambers and proportional counters, where the art has been highly developed, the articles can be and are elegantly presented. For the topics covering scintillation and Cerenkov counters and high-energy neutron detectors—fields where important contributions are yet to be made—it is difficult to give an elegant presentation. Here the authors rely mainly on quoting the published literature and pointing out the inconsistencies which are typical in a rapidly developing field. The article on nuclear emulsions deserves mention as it is a very clear and logical exposition of

this important technique in nuclear physics.

It is important for workers in the field of neutron physics to note that the mobility of electrons in  $\text{BF}_3$ , as given by Korff, is an order of magnitude too large. This value had its origin with Rossi and Staub [Ionization Chambers and Counters (McGraw-Hill, New York, 1949)] and caused a great deal of confusion in the United States. Barschall, in his otherwise complete article on the detection of neutrons, does not discuss this point.

This section of volume 45 of the *Handbuch* should prove valuable to students and researchers in experimental physics. However, its high cost (\$25) puts it out of reach of most individuals and relegating it to the library reference shelves.

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## New Books

*Advances in Chemical Physics*, vol. I. I. Prigogine, Ed. Interscience, New York, 1958. 425 pp. \$11.50.

*An Anthropologist at Work*. Writings of Ruth Benedict. Margaret Mead. Houghton Mifflin, Boston, 1959. 605 pp. \$6.

*Applied Medical Library Practice*. Thomas E. Keys, with chapters by Catherine Kennedy and Ruth M. Tews. Thomas, Springfield, Ill., 1958. 514 pp. \$10.75.

*Basic Organic Chemistry*. Louis F. Fieser and Mary Fieser. Heath, Boston, 1959. 372 pp. \$6.

*Le Bruit de fond*. Pierre Grivet and Austin Blaquier. Masson, Paris, 1958. 505 pp. Paper, F. 6500.

*Calculateurs analogiques repetitifs*. Rajko Tomovic. Masson, Paris, 1958. 186 pp. F. 3000.

*Centaur*. Essays on the history of medical ideas. Felix Marti-Ibanez. M.D. Publications, New York, 1958. 731 pp. \$6.

*Communicable Diseases Transmitted Chiefly through Respiratory and Alimentary Tracts*, vol. IV of *Preventive Medicine in World War II*. John Boyd Coates, Jr., Ed. Office of the Surgeon General, Department of the Army, Washington, D.C., 1958 (order from Supt. of Documents, GPO, Washington 25). 565 pp. \$5.50.

*Concise Dictionary of Judaism*. Dagobert D. Runes, Ed. Philosophical Library, New York, 1958. 237 pp. \$5.

*Cytodifferentiation*. Dorothea Rudnick, Ed. Univ. of Chicago Press, Chicago, Ill., 1958. 158 pp. \$3.75.

*Dairy Handbook and Dictionary*. J. H. Frandsen, Ed. Published by the editor, Amherst, Mass., 1958. 853 pp. \$10.50.

*Einführung in die Makromolekulare Chemie*. Hans Batzer. Huthig, Heidelberg, Germany, 1958. 234 pp. DM. 19.80.

*L'Exploration des galaxies voisines*. Par les methodes optiques et radio-electriques. Gerard de Vaucouleurs. Masson, Paris, 1958. 154 pp. Paper, F. 1600.

*Evolution of the Speech Apparatus*. E. Lloyd DuBrul. Thomas, Springfield, Ill., 1958. 113 pp. \$4.75.

*Fast Reactions in Solids*. F. P. Bowden and A. D. Yoffe. Academic Press, New York; Butterworths, London, 1958. 173 pp. \$7.

*Fundamentals of Advanced Missiles*. Richard B. Dow. Wiley, New York; Chapman & Hall, London, 1958. 583 pp. \$11.75.

*Geologie de l'uranium*. Marcel Roubaud. Masson, Paris, 1958. 462 pp. F. 6000.

*The Geology of Uranium*. Translated from Russian. Supplement No. 6 of the *Soviet Journal of Atomic Energy*, Atomic Press, Moscow, 1957. Consultants Bureau, New York, 1958. 134 pp. \$6.

*George Perkins Marsh, Versatile Vermonter*. David Lowenthal. Columbia Univ. Press, New York, 1958. 454 pp. \$6.50.

*Growth and Perfection of Crystals*. Proceedings of an International Conference on Crystal Growth held at Cooperstown, N.Y., on 27-29 August 1958. Sponsored by Air Force Office of Scientific Research, Air Research and Development Command, and General Electric Research Laboratory. R. H. Doremus, B. W. Roberts, David Turnbull, Eds. Wiley, New York; Chapman & Hall, London, 1958. 627 pp. \$12.50.

*A Handbook on Torsional Vibration*. E. J. Nestorides. Cambridge Univ. Press, New York, 1958. 686 pp. \$10.50.

*Pecos, New Mexico: Archaeological Notes*. Papers of the Robert S. Peabody Foundation for Archaeology, vol. 5. Alfred Vincent Kidder. Roberts S. Peabody Foundation, Phillips Acad., Andover, Mass., 1958. 380 pp. \$7.50.

*Topics in Electromagnetic Theory*. Dean A. Watkins. Wiley, New York; Chapman & Hall, London, 1958. 127 pp. \$6.50.

*Vapor-Liquid Equilibrium*. Eduard Hala, Jiri Pick, Vojtech Fried, Otakar Vilim. Translated by G. Standart. Pergamon, New York and London, 1958. 420 pp.

*Vacuum Metallurgy*. Rointan F. Bunshah, Ed. Reinhold, New York; Chapman & Hall, London, 1958. 490 pp. \$12.50. This volume contains the lectures presented during the course on vacuum metallurgy sponsored by the department of metallurgical engineering in cooperation with the Office of Special Service to Business and Industry, New York University, 10-14 June 1957. The lectures are on vacuum equipment, thermodynamics and kinetics, arcs and arc melting processes, induction melting processes, electron bombardment melting techniques, degassing in the liquid solid states, distillation of metals, metallurgical application of vacuum processing, analytical techniques, and future trends.

*Vitamins and Hormones*. Advances in research and applications, vol. 16. Robert S. Harris, G. F. Marrian, Kenneth V. Thimann. Academic Press, New York, 1958. 448 pp. \$11.60.

*Wild Paradise*. The story of the Coto Donana expeditions. Guy Mountfort. Houghton Mifflin, Boston, Mass., 1958. 240 pp. \$7.

## Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, but to the publisher or agency sponsoring the publication.)

*Certification of School Librarians*. A compilation of state requirements, 1958. Bull. 1958, No. 12. Mary Helen Mahar. 73 pp. \$0.30. *Foreign Language Laboratories in Schools and Colleges*. Bull. 1959, No. 3. Marjorie C. Johnston and Catherine C. Seerley. 86 pp. \$0.35. U.S. Office of Education, Washington, D.C., 1958 (order from Supt. of Documents, GPO, Washington 25).

*A Study of the Chironomidae (Diptera) of Africa South of the Sahara*, pt. IV. Bulletin, Entomology, vol. 6, No. 11. Paul Freeman. 102 pp. 30s. *The African Species of Stivalius, a Genus of Siphonaptera*. Bulletin, Entomology, vol. 7, No. 2. F. G. A. M. Smit. 37 pp. 10s. *Revisions of Mallophaga Genera. Degeeriella from the Falconiformes*. Bulletin, Entomology, vol. 7, No. 4. Theresa Clay. 86 pp. 30s. *Revision du Genre Exocentrus Mulsant (Col., Cerambycidae)*. S. Breuning. Bulletin, Entomology, vol. 7, No. 5. S. Breuning. 119 pp. *New and Little Known Emesinae (Reduviidae, Hemiptera) in the British Museum (Natural History)*. London. Bulletin, Entomology, vol. 7, No. 6. P. Wygodzinsky. 18 pp. 6s. *New Species and Subspecies of Odonata and on Some Trichoptera from Southern Rhodesia and Portuguese East Africa*. Bulletin, Entomology, vol. 7, No. 7. D. E. Kimmins. 22 pp. 7s. *A Revision of the Genera Nidalia and Bellonella, with an Emendation of Nomenclature and Taxonomic Definitions for the Family Nidaliidae (Oscorallia, Aleyonacea)*. Bulletin, Zoology, vol. 5, No. 5. Huzio Utinomi. 22 pp. 7s. *The Upper Permian Flora of England*. Bulletin, Geology, vol. 3, No. 9. Hilda M. Stoneley. 44 pp. 17s. 6d. British Museum (Natural History), London, 1958.

*Climate and Man in the Southwest*. A symposium held before the 33rd annual meeting of the Southwestern and Rocky Mountain Division of the AAAS, 30 April 1957, Tucson, Ariz. Terah L. Smiley, Ed. Univ. of Arizona Press, Tucson, 1958, 84 pp.

*Year Book of the Physical Society, 1958*. Containing special lectures delivered before the society, exhibition discourses, obituaries, proceedings of the meetings of the society, report of Council for 1957, accounts and balance sheet for 1957, and instructions to authors. Physical Society, London, S.W.7, 1958. 106 pp.

*Contributions to Stratospheric Meteorology*. GRD Research Notes No. 1. ASTIA Document No. AD 152626. Peter Antanaitis et al. 134 pp. *Project Prairie Grass, a Field Program in Diffusion*, vol. II. Geophysical Research Papers, No. 59. Morton L. Barad, Ed. 209 pp. Atmospheric Analysis Laboratory, Geophysics Research Directorate, Air Research and Development Command, Bedford, Mass., 1958 (order from U.S. Department of Commerce, Office of Technical Services, Washington 25).

*Herbaceous Angiosperms of the Lesser Antilles*. Ismael Velez. Biology Dept., Inter-American Univ. of Puerto Rico, San Juan, 1957. 121 pp.



# Reports

## Maximum Permissible Body Burden of Strontium-90

**Abstract.** The permissible body burden of  $\text{Sr}^{90}$  is discussed with respect to the mode of intake. It appears that the maximum permissible load may depend on the type of exposure, acute or chronic, the acute being the more serious.

The purpose of studying the distribution and metabolism of bone-seeking isotopes is to make it possible to derive the dose rates (local and average) to different parts of the skeletal tissue. In this way it may be possible in the end to establish values for the maximum permissible body burdens of various bone-seeking isotopes. This procedure includes a comparison of the derived dose rates with those judged to be necessary and sufficient for the production of various biological effects, such as malignant blood and bone changes. This seems at present to be a possible means of hazard evaluation, when lack of data makes direct comparisons difficult between the body burdens of different isotopes and the resulting biological effects.

The foregoing argument is general in that it applies to most bone-seeking isotopes. A general treatment of the principles for dose-rate calculations for such isotopes has recently been given (1). Specific isotopes have, furthermore, been treated in several works. In the following discussion (2)  $\text{Sr}^{90}$  will be chosen to illustrate the method proposed for estimating maximum permissible body burdens.

It has been calculated (1) that the skeletal tissues receive a dose rate of 2.6 rem/year (or 7.25 mrem/day) on the average, from a total body burden of 1  $\mu\text{C}$  of homogeneously distributed  $\text{Sr}^{90}$ .

This assumes a homogeneous dispersion of the  $\text{Sr}^{90}$  in the 7000 g of skeletal tissue (the standard man). A body burden of 1  $\mu\text{C}$  of  $\text{Sr}^{90}$  is equivalent to 1000 S.U. (sunshine units, or  $\mu\text{C}$  of  $\text{Sr}^{90}$  per gram of Ca).

It is obvious from several investigations that the assumption of a homogeneous  $\text{Sr}^{90}$  distribution is unrealistic, since  $\text{Sr}^{90}$  occurs both as a general diffuse labeling and in localized sites (reactive bone) of microscopic and macroscopic dimensions. If the dose rates are calculated in relation to the isotopic content of the reactive bone sites, where a great deal of  $\text{Sr}^{90}$  is found, more realistic data may be attained. It is therefore necessary to consider the geometrical distribution of these reactive sites.

The geometrical configuration may be accounted for by two extreme cases, the cylindrical geometry of the Haversian systems and the more irregular appearance of the labeling in spongy bone. Estimates show that spongy bone is often the more important tissue when high dose rates are considered. However, it is an impossible task to make exact dose calculations for the spongy bone. A simplified geometry has to be assumed, and this has been done in a recent monograph (1) in which a system of plane parallel slabs of reactive bone interspersed with regions of bone marrow is considered. It is found that a homogeneous contamination of the reactive bone slabs with 1  $\mu\text{C}$  of  $\text{Sr}^{90}$  per gram of bone may give maximum dose rates of 25 to 35 mrem/day to bone and bone-marrow cells, with corresponding average values of 15 to 20 and 12 to 16 mrem/day. It is now important to remember that the reactive bone constitutes a certain fraction of the 7000 g of bone tissue that the standard man is supposed to contain. This fraction varies with the state of growth and remodeling of the bone tissue. It thus differs for children and adults as well as for different bones and parts thereof within the same skeleton. It appears legitimate to assume a fraction of around 0.20 to 0.25 to be a representative average figure. Thus, 1400 to 1750 g of the skeleton is considered to be reactive. In adults it can be estimated that approximately half of the reactive bone is found as spongy bone where the major part of the blood-forming bone marrow is situated.

If the maximum permissible value of

1  $\mu\text{C}$  of  $\text{Sr}^{90}$  is considered, the following dose rates are obtained: 7000 g of bone will receive more than 1.8 mrem/day and, on the average, 5.5 mrem/day; 700 to 900 g of spongy bone will, on the average, receive 10 mrem/day—in some parts, 21 mrem/day; 1500 g of red bone marrow will receive more than 1.8 mrem/day and, on the average, 7.5 mrem/day; 1100 g of red bone marrow will receive an average of 9 mrem/day—in some parts, 21 mrem/day.

The maximum-dose-rate values given above are based on one assumption which is obviously not fulfilled—namely, that of homogeneous labeling of the different reactive sites or "hot spots" in relation to each other. However, variations in the  $\text{Sr}^{90}$  concentrations are to be considered rather the rule than an exception. Regions of the same dimensions as the average range of  $\text{Y}^{90}$  beta particles ( $\sim 2$  mm) and containing a large number of hot spots may differ from the average in  $\text{Sr}^{90}$  concentration by a factor of 5 to 10. This implies a corresponding increase in the maximum dose rates to some parts of the bone tissue.

It should finally be pointed out that the simplified geometry used to depict the spongy bone may lead to dose-rate values that are too low in comparison with those for biological material. It is easy to conceive of geometrical arrangements (for example, bone lamellae meeting in a corner or inhomogeneous activity distribution within hot spots) which will give locally a two- to threefold increase in the maximal dose rate. A body burden of 1  $\mu\text{C}$  of  $\text{Sr}^{90}$  acquired in one exposure may thus give dose rates on the microscopic level as high as 300 to 500 mrem/day to certain parts of the reactive bone tissue and the bone marrow.

It seems advisable to correlate the permissible body burden with the maximum local dose rates rather than with the average whole-body values. The production of bone and blood malignancies is probably dependent on the local microscopic conditions—that is, on the local dose rates. Thus, when we compare the dose rate of 300 to 500 mrem/day from 1  $\mu\text{C}$  of  $\text{Sr}^{90}$  with the dose of 1000 rem which is considered after a relatively long period to be significant in the production of biological damage to bone tissue, it is apparent that this dose (1000 rem) may be attained within 10 years. This suggests a revision of the value for the maximum permissible body burden of  $\text{Sr}^{90}$  to 0.1  $\mu\text{C}$  instead of the currently accepted value of 1  $\mu\text{C}$ , which was derived from comparison with radium data.

The foregoing argument is concerned with acute or short-time poisoning with  $\text{Sr}^{90}$ . If  $\text{Sr}^{90}$  is taken into the body over a considerable period, the situation is different, as the pattern of isotopic localization becomes different. Such "chronic poisoning" conditions prevail today for

*Instructions for preparing reports.* Begin the report with an abstract of from 45 to 55 words. The abstract should not repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper. (Since this requirement has only recently gone into effect, not all reports that are now being published as yet observe it.)

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to one 2-column figure (that is, a figure whose width equals two columns of text) or to one 2-column table or to two 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [Science 125, 16 (1957)].

children born in recent years, as essentially all calcium available in the biosphere is, and will be, contaminated with  $\text{Sr}^{90}$  from weapon tests.

If in the first instance considered above a uniform contamination is assumed, 7000 g of bone will receive an average dose of 7.25 mrem/day. This dose rate is maximal to red bone marrow, which on the average receives 1.5 mrem/day. The degree of mineralization varies, however, and it is found, on comparing regions of the same dimensions as the average beta range ( $\sim 2$  mm), that there may be calcium concentrations which differ from the average by a factor of 2 to 3. This implies that certain parts of the bone and bone marrow may receive doses of around 15 mrem/day.

It is apparent, however, that a constant intake of  $\text{Sr}^{90}$  will not give a non-uniform contamination of the skeleton in the sense described above. The mechanism of remodeling and exchange gives rise to a biological half-life for  $\text{Sr}^{90}$  in the skeleton, and the radioactive decay has some influence, as well, on the equilibrium state. If we assume an effective half-life of 2700 days (7.4 years), the  $\text{Sr}^{90}$  concentration may be expected to vary from the average by a factor of about 2 for a 15-year period and by successively larger factors for longer periods. To some extent this fact is accounted for by the differences in mineralization. Therefore in the case of chronic poisoning with  $\text{Sr}^{90}$  a higher figure than  $0.1 \mu\text{c}$  is tolerable as total body burden; tentatively, the  $1 \mu\text{c}$  level may be considered to be tolerable.

Today the  $\text{Sr}^{90}$  contamination of the geosphere and the biosphere is steadily increasing. This corresponds to a situation with aspects that lie somewhere between those of acute and chronic  $\text{Sr}^{90}$  poisoning. Children in the 0- to 5-year age group are examples of individuals with chronic poisoning conditions. Adults above 20 years of age are more likely to be examples of acute poisoning.

It should finally be pointed out that the conditions described here in relation to  $\text{Sr}^{90}$  have their counterpart for other isotopes. For instance, it seems that the evaluation of the hazards from radium poisoning should take into account the difference between acute and chronic poisoning. This is the more advisable since the short range of Ra alpha particles will cause greater differences in the local dose rates than is the case with  $\text{Sr}^{90}$ .

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## Notes

1. An extensive bibliography is to be found in A. Engström *et al.*, *Bone and Radiostrontium* (Almqvist and Wiksell, Stockholm, 1958).
2. This article is part 3 of a series on health hazards from fission products and fallout; for part 1, see K. Low and R. Björnerstedt, *Arkiv Fysik* 13, 85 (1957); part 2, by R. Björnerstedt, is in preparation.

27 October 1958

## Release of Autonomic Mediators in Cardiac Tissue by Suprathreshold Stimulation

**Abstract.** Pharmacological evidence is presented supporting the theory that the increase in contractile strength of isolated cardiac muscle under suprathreshold stimulation is due to the release of an adrenergic mediator (norepinephrine). However, release of this material does not account for changes in contractile strength associated with changes in frequency of stimulation at threshold levels.

Whelan *et al.* have reported that during periods of suprathreshold stimulation (stimulation at voltages well above threshold) contractile force of isolated cat papillary muscle gradually increases, while that of the cat atrial strip gradually decreases, or first decreases and then increases (1). They tentatively attributed these alterations in force to release of autonomic mediators by the suprathreshold stimuli, release of acetylcholine mediating a decrease, and release of "one of the epinephrine compounds" mediating an increase. A similar postulate had been made previously by Nelemans (2) and by Ursillo (3) to explain force changes following short periods of tetanic stimulation of spontaneously beating frog heart and mammalian atria, respectively.

Several years ago, while working with isolated left atria, we encountered the same phenomena reported by Whelan (4). With guinea-pig atria at both  $27^\circ$  and  $37^\circ\text{C}$ , suprathreshold stimulation increased contractile strength (Fig. 1B), whereas with rat atria at  $27^\circ\text{C}$ , it produced either a decrease or a decrease followed by an increase. Since the decrease in contractile strength produced in rat atria could be blocked by atropine and potentiated by physostigmine, we concluded that it was mediated by released acetylcholine. However, we were unsuccessful in our early attempts to obtain convincing pharmacological evidence that the increase in contractile strength produced in guinea-pig atria was due to release of adrenergic mediator, since none of several adrenergic blocking agents, including dibenamine, available to us at the time produced a clear-cut blockade of the increases in strength elicited by epinephrine and norepinephrine.

During the past year we have returned

to the problem of identifying the potentiating material released by suprathreshold stimulation of guinea-pig left atria, making use of two newer pharmacological agents. One of these agents is 1-(3',4'-dichlorophenyl)-2-isopropylaminoethanol hydrochloride (DCI), which recently has been reported to be an effective blocking agent against both the inhibitory effect of sympathomimetic amines on certain smooth muscles and the stimulating effects of these amines on the heart (5). The second agent is reserpine, which causes a rapid and drastic depletion of the endogenous catechol amines (chiefly norepinephrine) of cardiac tissue when it is administered to animals at high dose levels (6).

Exposure of guinea-pig left atria to about  $10^{-4}$  g of DCI per milliliter for 10 to 20 min effectively antagonized the positive inotropic action of epinephrine and norepinephrine (Fig. 1, A and C). The antagonism persisted for long periods after washout of the DCI, whereas certain undesirable side effects of DCI at the concentration used—such as de-

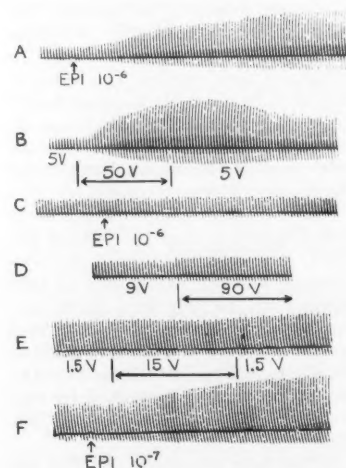


Fig. 1. Effects of suprathreshold stimulation and of epinephrine on contraction amplitude of isolated left atria of guinea pig under various conditions. Recordings were made with an ink-writing isotonic lever, exerting 1g tension on the atrium, which was suspended in 20 ml of oxygenated Krebs bicarbonate solution ( $\text{pH}$  7.4) at  $27^\circ\text{C}$ . The stimulus was provided by a Grass 4C stimulator (biphasic pulse, 1 to 2 msec duration) at a frequency of 1 per second through Ag-AgCl electrodes. A, Effect of epinephrine on atrium from a normal guinea pig; B, effect of suprathreshold stimulation on the same atrium; C, effect of epinephrine on the same atrium after treatment with  $10^{-4}$  DCI for 10 min; D, effect of suprathreshold stimulation on the same atrium after DCI treatment; E and F, effects of suprathreshold stimulation and of epinephrine on atrium from a reserpinized guinea pig.

pressed contractile strength, increased refractory period and threshold, and partial conduction block—largely disappeared within about 30 min after washout (7). Therefore, quantitative testing for adrenergic blockade was usually carried out about 30 min after washout of DCI. Despite its persistence, the blockade appeared to be of the reversible competitive type, susceptible to “break-through” by very high concentrations ( $\sim 10^{-5}$  g/ml) of epinephrine and norepinephrine. That the blockade was relatively specific was indicated by the fact that DCI did not significantly antagonize the positive inotropic effects of added  $\text{Ca}^{++}$  or strophanthin. On the other hand, the increase in strength with suprathreshold stimulation was antagonized by DCI as effectively as was that due to added epinephrine or norepinephrine (Fig. 1, B and D). This finding strongly supports the hypothesis that the increase with suprathreshold stimulation is mediated through release of some adrenergic substance (probably norepinephrine).

Additional strong evidence for the hypothesis was obtained with atria from guinea pigs which had been pretreated with reserpine (approximately 1 to 5 mg/g day intraperitoneally for 1 to 2 days) in order to deplete cardiac catechol amines. Such atria still responded well to added epinephrine or norepinephrine, but with suprathreshold stimulation they now gave either no significant increase in strength or a slight decrease (occasionally followed by a slight delayed increase) (Fig. 1, E and F). It should be noted that Burn has similarly used reserpine to show that the stimulating effect of nicotine on isolated rabbit atria is actually due to adrenergic mediator released by this agent (8). We have confirmed Burn's results with nicotine, using normal and reserpinized guinea-pig atria, and in addition have found that DCI can block the positive inotropic action of nicotine.

In preliminary experiments on cat papillary muscle, DCI and reserpine have yielded results completely analogous to those reported above for left atria of guinea pigs. Thus, we directly support Whelan's hypothesis that the increase in contractile force of papillary muscle with suprathreshold stimulation is due to the release of “one of the epinephrine compounds.” However, we must disagree strongly with Whelan's suggestion that “epinephrines” and acetylcholine, released at each spontaneous beat or electrically driven beat at threshold voltages, are responsible for such phenomena as positive treppe and poststimulation potentiation. Such phenomena are still strikingly demonstrable in both guinea-pig atria and cat papillary muscles when potentiation due to suprathreshold stimulation has been elimi-

nated by DCI or reserpine (9). Also, negative treppe in isolated rat atria is not altered by atropine blockade of the depression due to suprathreshold stimulation.

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12 September 1958

### Earth Oblateness in Terms of Satellite Orbital Periods

**Abstract.** A theoretical equation relating the earth's oblateness to the anomalistic and nodal periods and orbit parameters of an earth satellite is presented. In the absence of exact data on nodal periods, Vanguard prediction data are utilized to obtain a check calculation for the oblateness and to establish the validity of the method.

The utility of artificial earth satellites for independent determination of the earth's oblateness by means of observations of the secular perturbations of the orbit [precession of the node and of perigee (1)] is well recognized. In the course of studies of such orbit perturbations, I have derived an equation for the difference between the anomalistic period  $T_A$  (perigee to perigee) and nodal period  $T_N$  (node to node) which should

provide an additional independent means for determining the oblateness. Specifically,

$$T_A - T_N = \frac{JR^2 r_o T_o}{a^3 (1 - e^2)^{5/2}} \left( 2 - \frac{5}{2} \sin^2 i \right) \\ = \frac{2\pi JR^2 r_o^2}{(GM)^{1/2} a^{5/2} (1 - e^2)^{5/2}} \\ \times \left( 2 - \frac{5}{2} \sin^2 i \right) \quad (1)$$

where  $J$  is the coupling constant in the oblate earth's potential (2),  $R$  is the equatorial radius of the earth,  $r_o$  is the distance from the center of the earth to the satellite at the node,  $T_o$  is the satellite period for an assumed spherical earth,  $G$  is the constant of gravitation,  $M$  is the mass of the earth,  $a$  is the semi-major axis of the orbit,  $e$  is the orbital eccentricity, and  $i$  is the angle of inclination of the orbit to the plane of the equator.

Because of the precession of the line of apsides (3)  $r_o$  will range in magnitude from a perigee distance of  $(1 - e)a$  to an apogee distance of  $(1 + e)a$ ; and we come to the interesting conclusion that the difference between the anomalistic and nodal periods will exhibit a cyclic variation whose period is equal to that of the precessional motion of the line of apsides. The extremes of  $(T_A - T_N)$  are then

$$(T_A - T_N)_{\min.} = \frac{JR^2 T_o (1 - e)^2}{a^2 (1 - e^2)^{5/2}} \\ \times \left( 2 - \frac{5}{2} \sin^2 i \right) \\ = \frac{2\pi JR^2 (1 - e)^2}{(GM)^{1/2} a^{1/2} (1 - e^2)^{5/2}} \\ \times \left( 2 - \frac{5}{2} \sin^2 i \right) \quad (2)$$

which can differ by as much as 20 seconds for orbits of low inclination. Note that for  $\sin^2 i = 4/5$  the difference in periods vanishes, which is in accord with the fact that at this inclination,  $63.5^\circ$ , the line of apsides neither advances nor regresses (3). For  $i$  less than  $63.5^\circ$ , the anomalistic period will be greater than the nodal period; and, conversely, at larger inclinations the nodal period will be the greater. In any case, the difference in periods increases with orbit eccentricity, and only for near-circular orbits ( $e=0$ ) will there be no cyclic variations in  $(T_A - T_N)$ . Unlike the motions of the node and perigee, which fall off rapidly with distance,  $(T_A - T_N)$  falls off very slowly with increasing orbit size; and hence the effect should be detectable even for orbits at great distance.

Equation 1 has been developed to first-order in the oblateness parameter  $J$  on the presumption of no drag or other perturbing factors. For satellites with perigees above the region of sensible atmospheric drag the orbit parameters should be measurable to great accuracy, which



in turn will be reflected in precise determination of  $J$ . Even for orbits in which atmospheric drag will slightly perturb the periods, the alteration in  $T_A$  should be very closely the same as that in  $T_N$ , so that the difference should be relatively unaffected by drag.

With the exception of satellite 1958 $\beta$ 2 (Vanguard I), the existing satellite orbits are too seriously affected by drag for the above equations to be precisely applicable; and even for Vanguard I precise data on nodal periods have not yet been published. In the absence of such information one can utilize the predicted times of equator crossings, as issued by the Vanguard Computing Center and the Naval Research Laboratory, to derive approximate nodal periods. In this manner values of  $(T_A - T_N)$  were calculated and plotted as a function of equator pass number in Fig. 1. The predicted cyclic variation, with minima and maxima corresponding to perigee and apogee occurrences near the node, is indeed evident. The lack of complete symmetry in the curve is a consequence of the fact that values of the nodal period were interpolated to the nearest 0.1 second, while the predicted times of equator crossings were given only to the nearest second.

On the basis of the Vanguard prediction data and Eq. 1, a value for the oblateness parameter has been calculated as  $J = 0.001631 \pm 0.000031$ . In turn this corresponds to an earth oblateness (2) of  $1/297.6 \pm 2.7$ . This is to be compared with the international value of  $1/297.0$  and O'Keefe's preliminary value (4) of  $1/298.3 \pm 0.1$ , which was derived from the secular motions of the node and perigee of satellite 1958 $\beta$ 2.

Obviously, in spite of the near agreement between the value herein obtained and the other quoted values for the oblateness, little significance can be attached to this figure because of the associated large statistical probable error. However, even from these approximate

calculations, it is clear that a significant check on the validity of the theory has been obtained. Since no method for independent determination of the earth's oblateness should be left unexplored, it is urged that every effort be made by tracking stations and computation centers to determine satellite anomalistic and nodal periods accurately.

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1. Those points on the celestial sphere where the satellite crosses the equator are called the nodes. The point on the orbit nearest the earth is called the perigee, while the major axis, extended indefinitely, is called the line of apsides.
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19 November 1958

### Inhibition of Growth of Excised Tomato Roots by 2-Diethylaminoethanol

**Abstract.** Approximately 40  $\mu$ M 2-diethylaminoethanol (DEAE) caused 50-percent inhibition of growth of the main axis. Inhibition was relieved by 2-dimethylaminoethanol and choline but not by ethanolamine. A marked morphogenetic effect of DEAE is attributed to differences in sensitivity of main and lateral meristems and to an effect upon the postulated hormonal system controlling apical dominance.

The work reported here forms part of a project designed to study the replacement of vitamin B<sub>6</sub> by ethanolamine (2-aminoethanol) in the nutrition of excised tomato roots (1).

Ethylation of the amino group of ethanolamine gives compounds which, because of their structural similarity to the metabolites 2-mono-, 2-di-, and 2-trimethylaminoethanol (choline), are possible antimetabolites which could be useful in the study of the metabolism of ethanolamine. No reports of the effects of such ethylated derivatives on plant growth are known to me, although some indication of the value of these compounds as inhibitors is given from work with animals. Thus the incorporation of the ethyl carbon of ethionine into choline and creatine of rat tissue (2) suggested that growth inhibition by ethionine could be due, at least in part, to inhibitory effects of ethyl analogs of choline or substances containing choline. Subsequently (3) it was shown that triethylcholine in-

hibited the growth of rats. The inhibition was relieved by choline and, to a lesser extent, by methionine.

2-Diethylaminoethanol is not a proven antimetabolite. However it forms part of the structure of a number of drugs, and, consequently, it has been studied by animal physiologists who have reported various pharmacological effects (4). It is not known whether any such effects are due to interference with choline metabolism. However, in a study of the oxidation of choline-like substances by rat-liver preparations (5) it was found that, although choline and a number of structural analogs were oxidized, DEAE was not oxidized. Furthermore, DEAE gave a 25-percent depression of choline oxidation by the homogenate. All these results suggested that DEAE might be valuable as an inhibitor *in vivo*.

A sample of DEAE was given to me by the Jefferson Chemical Company (New York). This report describes the inhibitory effects of DEAE on growth of excised tomato roots grown in sterile culture and some nutritional experiments on the reversal of the inhibition. The clone of excised tomato roots used as a source of inocula is designated R5 (6). The general experimental techniques, and some cultural requirements of the clone, are described elsewhere (6, 7). All additions to the basal medium used here were autoclaved in the medium. The measurements recorded are of roots grown for 6 days from 10-mm tips and are measurements of the final length of the main axis per root and of the total length of the ten basal laterals per root. The number of laterals per root in all experiments was found to be proportional to the length of the main axis and is, therefore, omitted from the results.

The effects of a range of concentrations of DEAE on the growth of roots is shown in Fig. 1. There was a marked difference between the growth response to DEAE of the main axis and of the lateral roots. The inhibition of lateral growth at low concentrations was relieved at higher concentrations which inhibited growth of the main axis. The explanation of this differential effect probably lies in the observation (8) that apical dominance (inhibition of lateral roots by the main apex) is manifested in excised tomato roots. In the presence of a growth inhibitor, the growth of lateral roots will be controlled by the inhibitions due to both the exogenous inhibitor and the factors causing apical dominance. Presumably, at those concentrations of DEAE which relieved the inhibition of lateral growth caused by low concentrations of DEAE, the simultaneous inhibition of main axis growth resulted in a removal of the factors causing apical dominance. This, in turn, led to an in-

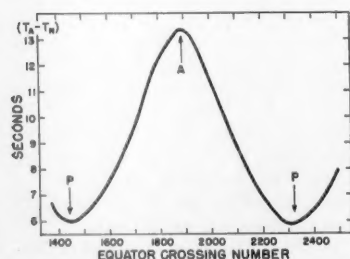


Fig. 1. Difference between anomalistic and nodal periods as a function of equator pass number for satellite 1958 $\beta$ 2. Approximate perigee (P) and apogee (A) occurrences at the equator are indicated by arrows (5).

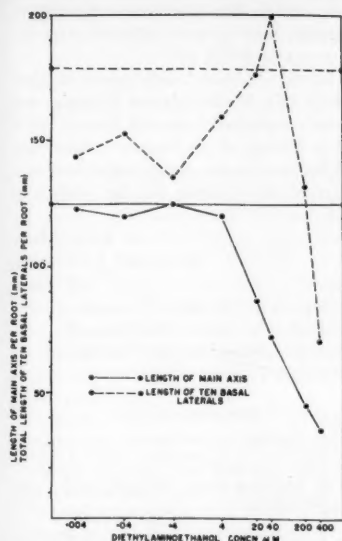


Fig. 1. Effect of 2-diethylaminoethanol on excised tomato roots grown for 6 days from 10-mm tips. Each point is the mean of nine roots. The horizontal lines show the growth in the control.

Table 1. The effect of either choline (experiment 1) or 2-dimethylaminoethanol (DMAE) (experiment 2) on inhibition by 2-diethylaminoethanol (DEAE). The values for experiment 1 are means of nine roots; for experiment 2 the values are the means of 19 roots in the control and low DEAE series and of nine roots in the high DEAE series.

Additions			Feature measured	
DEAE ( $\mu$ M)	Choline ( $\mu$ M)	DMAE ( $\mu$ M)	Main axis length (mm)*	Lateral axis length (mm)†
<b>Experiment 1</b>				
0	0		133.1	200.5
40	0		79.4	202.2
40	.04		81.2	191.6
40	.12		81.5	175.1
40	.4		88.6	218.8
40	1.2		128.4	161.9
40	4.0		96.2	196.1
40	12.0		87.1	183.0
<b>Experiment 2</b>				
0	0		127.4	150.1
40	0		69.7	147.8
40	3.2		83.7	157.1
40	4.0		84.1	158.3
40	8.0		87.7	153.2
40	12.0		77.8	172.4
200	0		36.3	84.8
200	3.2		43.8	109.6
200	4.0		47.1	112.5
200	8.0		50.2	130.0
200	12.0		53.3	104.6

\* Length of main axis per root.

† Total length of ten basal laterals per root.

crease in growth of lateral roots. In other words, the inhibition of apical dominance more than offset the inhibition of lateral growth by DEAE.

Because of the differences in the proportions of the lateral roots and the main axis, the difference in morphology was very striking between control roots and those grown in 40- $\mu$ M DEAE. The dose-response curves obtained with DEAE are not simply a consequence of inhibition as such, because they were not obtained with the antimetabolites sulfanilamide, ethionine, thiouracil, and desthiobiotin when these were added to the basal medium used here. However, dichloroaniline, an antiauxin (9), gave curves very similar to those shown in Fig. 1 (10). This suggests (i) that the inhibitory effect of DEAE at low concentrations is due to an effect upon the hormonal system which inhibits growth of lateral roots and (ii) that differences in the morphology of root systems (11) may result from differences between the endogenous concentrations at which an inhibitor affects the growth metabolism of terminal meristems and those at which it affects the metabolism upon which the hormonal, apical dominance depends.

Ethanolamine did not relieve the inhibition caused by DEAE at a concentration (40  $\mu$ M) giving approximately 50 percent inhibition in growth of the main axis. The concentrations of ethanolamine tested ranged from 0.12 to 400  $\mu$ M. These concentrations were not inhibitory when supplied in the absence of DEAE.

Neither choline nor 2-dimethylaminoethanol increased the growth of clone R5 when supplied in the absence of DEAE. The effects of choline or 2-dimethylaminoethanol on inhibition by DEAE are shown in Table 1. Inhibition was relieved, to a greater or lesser extent, by either of these substances. The low level at which they are toxic to excised tomato roots precludes any attempts to determine whether the antagonisms are competitive or otherwise. However, the results presented here show that 2-diethylaminoethanol may be a useful inhibitor in the study of ethanolamine and choline metabolism in plants, and in the study of apical dominance in excised tomato roots.

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## Partial Chromatographic Separation of Pentose- and Deoxypentosenucleic Acids

**Abstract.** From a mixture of tobacco mosaic virus pentosenucleic acid (RNA) and calf thymus deoxypentosenucleic acid (DNA) (2 mg each), 73 percent of the RNA was separated free of DNA by discontinuous elution chromatography with phosphate buffers at pH 6.7 on columns of calcium phosphate.

The presently available procedures for the separation of pentosenucleic acids from deoxypentosenucleic acids in complex biological materials have been discussed by Chargaff (1), together with their individual advantages and limitations. Included among these is the method of Zamenhof and Chargaff (2), which involves the preferential adsorption of RNA on activated charcoal. The recent development of procedures for chromatographic separations of DNA on calcium phosphate columns (3-5) has prompted the extension of such techniques to mixtures of RNA and DNA. This we would like to report in this paper (6).

The results obtained for the attempted separation of tobacco mosaic virus RNA (7) from calf thymus DNA (Worthington), in a mixture of the two acids, by means of this chromatographic adsorbent material are presented in Fig. 1. Chromatography of 2.0 mg tobacco mosaic virus RNA alone (Fig. 1A) results in a series of peaks eluted by 0.12, 0.14, 0.16 and 0.20M phosphate buffer eluents at pH 6.7. The ratio of optical absorbance of 280 m $\mu$  to the optical absorbance at 260 m $\mu$  ( $A_{280}/A_{260}$ ) of these eluates was 0.46. That of the original RNA solution placed on these columns was 0.46.

When a mixture composed of 2.0 mg of tobacco mosaic RNA and 2.0 mg of DNA is chromatographed (Fig. 1B), a series of major peaks, as evidenced by the corresponding  $A_{260}$  values of the eluates in tubes 31 to 120, appears. The eluates in tubes 31-79 contain RNA, since the  $A_{280}/A_{260}$  values are within the limits of  $0.46 \pm 0.01$ . Further, no significant

amounts of DNA were found by the Keck method (8) in any of these eluates. The DNA appears in the tubes beyond No. 79, mixed with the remainder of the RNA (that is, 27 percent) not accounted for in the eluents up to tube No. 79. The value of  $A_{490}$  of the DNA-indole "chromosol" produced by Keck's method parallels in percentage the value of  $A_{260}$  of respective eluates in tubes 70 to 112. This is shown by the shaded portions on this chromatogram (Fig. 1B). The distinct break in the nature of the peaks eluted after tube 79 is reflected in the change in the ratio  $A_{280}/A_{260}$  from 0.46 to  $0.545 \pm 0.01$ . The latter value is compatible with that of solutions of the DNA sample used (0.54).

From the data shown in Fig. 1B, the amount of RNA separated free of DNA was calculated to be 73 percent of the

total amount of RNA present in the initial mixture. These experiments were performed in duplicate and have been repeated with 1.0 mg of RNA in the place of 2 mg, with essentially the same results. In contrast to these results on calcium phosphate columns, it has been reported (9) that RNA was not resolved from a mixture of RNA and DNA on columns of ECTEOLA-cellulose.

It is to be noted from Fig. 1 that RNA (2.0 mg) desorbed from the column much earlier (that is, at lower eluent concentrations) in the presence of DNA than it did in the absence of DNA (2 mg). One reasonable explanation of this finding implies that, as the full capacity of the column is approached (but not necessarily attained) by the load of 4 mg of mixed nucleic acids, the RNA is preferentially displaced by the DNA. This

same displacement phenomenon also has been observed in the chromatography of proteins and of polynucleotides in the presence of DNA (10).

In experiments briefly reported elsewhere (5), bovine plasma albumin was cleanly separated from calf thymus DNA when 20 mg of the former and 2.0 mg of the latter were chromatographed as a mixture on columns of this absorbent (11).

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7. We are indebted to Dr. C. A. Knight, Virus Laboratory and Department of Biochemistry, University of California, Berkeley, for the gift of tobacco mosaic virus RNA, freshly prepared by the method of Gierer and Schramm.
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11. A report giving full details of the development, preparation, chemical and physical properties, and chromatographic characteristics of this calcium phosphate method is in preparation.

29 September 1958

#### Centrifugal Arousal in the Olfactory Bulb

**Abstract.** The electrical activity of the olfactory bulb was recorded in awake, unrestrained cats with electrodes permanently implanted. It was found that any kind of sensory stimulation producing alertness or arousal brought about the appearance of bursts of rhythmic activity, the magnitude of which was related to the degree of alertness of the cat.

Some centrifugal fibers terminating around second-order sensory neurons, such as those of the retina and of the olfactory bulb, have been known for a long time, since Cajal's anatomical descriptions (1). However, it has only recently been shown that stimulation of these fibers may modify the electrical activity of those neurons. Electrical stimu-

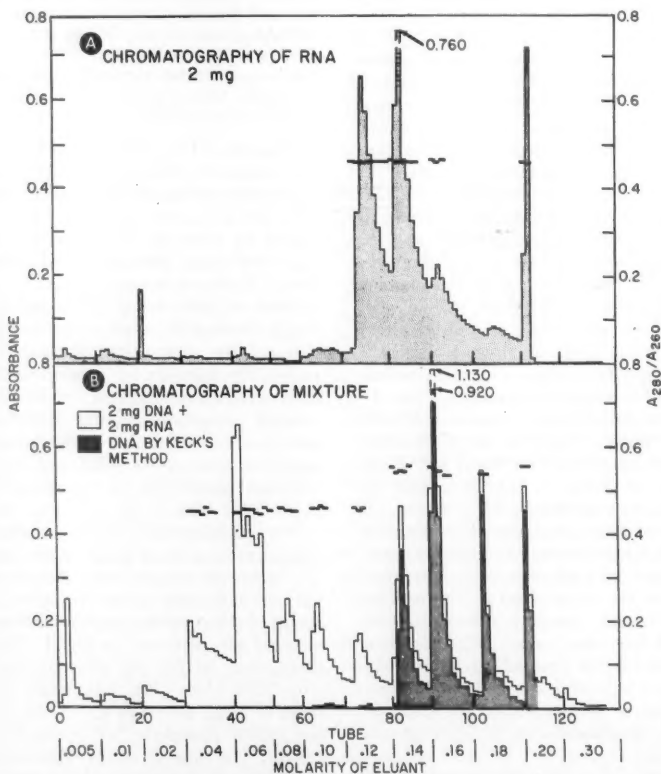


Fig. 1. A, Chromatography of tobacco mosaic virus RNA (2.0 mg). B, Chromatography of a mixture of tobacco mosaic virus RNA (2.0 mg) with calf thymus DNA (2.0 mg) on columns of modified calcium phosphate (1 by 7.5 cm) at 6° C. The solvent (4 ml volume) in which the nucleic acids were applied to the columns was 0.005M sodium phosphate buffer at pH 6.7; rate of elution, 4.8 ml/hr; tube volume 4.8 ml; discontinuous gradient elution. Eluents and phosphate buffers (pH 6.7) at molarities indicated. The value of  $A_{260}$  for each eluate is plotted according to the scale on the left. Values of bars, ( $A_{280}/A_{260}$ ) are plotted according to the scale on the right. In B, the darkly shaded areas represent respective concentrations of DNA expressed in terms of  $A_{490}$  of the DNA-indole "chromosol" plotted according to the scale at the left. For concentrations of DNA (in terms of  $A_{260}$ ) in the range between 0.40 to 1.15, the ratio  $A_{490}$  of the DNA-indole "chromosol" to  $A_{260}$  of a DNA solution was  $1.12 \pm 0.02$  for any particular concentration of DNA. Recovery of DNA: 82 percent of that placed on column.



lation of the reticular formation of the brain stem, involved in wakefulness and arousal, inhibited sensory transmission at second-order neurons of the spinal cord (2), of the gracilis nucleus (3), and of the spinal fifth sensory nucleus (4). Photoc impulses from the ganglion cells of the retina were either potentiated or inhibited (3, 5). As an apparent exception, the olfactory bulb was not shown to be controlled by centrifugal influences from the arousal system of the brain stem. Recruiting waves were recorded in the olfactory bulb during low-frequency stimulation of intralaminar thalamic nuclei (6), and suppression of olfactorily evoked activity was found during high-frequency electrical stimulation of basal rhinencephalic structures (7).

Pioneer exploration of the functional significance of those centrifugal influences coming from Magoun's "activating" system in the brain stem led to the discovery of sensory suppression of auditory signals at the cochlear nucleus during attention to other stimuli (8) and during habituation elicited by monotonous repetition of the same acoustic stimulus (9). Photoc retinal signals recorded at the optic tract are also diminished when attention is distracted by other environmental stimuli (10, 11), whereas the same signals are facilitated when attention is focused upon that particular photic stimulus (11). So far, the retina is the only place where secondary sensory neurons have been found to be facilitated during activation of the brain-stem arousal system.

In an attempt to explore the functional significance of the centrifugal fibers to the olfactory bulb in the cat, multipolar electrodes were permanently implanted in this structure (12). Bipolar electrodes were also implanted in the region of the anterior commissure, and in the mesencephalic reticular formation. The electrical activity from the unanesthetized, unrestrained cat was recorded with an ink-writer oscillograph or with a cathode-ray oscilloscope. In this paper, centrifugal augmentation of the electrical activity of the olfactory bulb is reported.

When the cat was awake but relaxed, a uniform low-voltage and fast-frequency activity was observed in the olfactory bulb, simultaneously with large, slow waves in the septal area. But when the cat was alerted by any environmental stimulus, simultaneously with a "desynchronization" of the tracing in the septal area, typical burst discharges of rhythmic (34 to 48 per second) activity appeared in the olfactory bulb. The magnitude and duration of the "arousal discharges" had a direct relationship to the degree of alertness or excitement of the animal. Often, the intermittency of these discharges varied between 20 and 25 per minute—that is, within the range of the

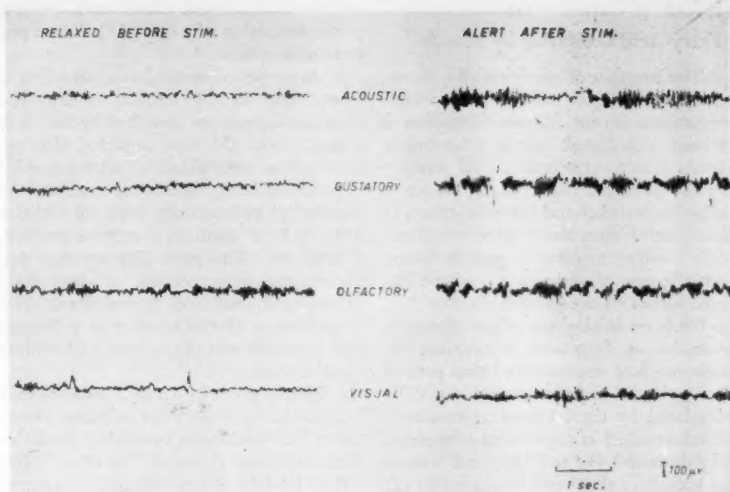


Fig. 1. Electrical activity of the olfactory bulb, showing the effects produced by stimuli of various sensory modalities which elicited behavioral alertness.

respiratory rate. However, no constant relationship was observed between any of the respiratory phases and the initiation of the "arousal discharges."

It must be emphasized that the observed increase of activity in the olfactory bulb was elicited not only by odors but also by visual, acoustic, somatic, or gustatory stimulation. Figure 1 shows the effects produced in the cat's olfactory bulb by stimulation of various sense modalities. The fact that stimuli of multiple sensory modalities were capable of yielding the same activating effect in the olfactory bulb suggested that the responsible centrifugal influence proceeds from a region of sensory convergence related to arousal. Indeed, electrical stimulation of the mesencephalic reticular formation (50 cy/sec during 2 to 3 sec) brought about, simultaneously with the alerting behavior of the cat, the appearance of "arousal discharges" in the olfactory bulb and "desynchronization" of the electrical activity in the septal area. On the other hand, central anesthesia or a lesion in the mesencephalic reticular formation which rendered the cat unconscious eliminated and prevented the appearance of "arousal discharges" through any kind of sensory stimulation.

From the above-mentioned experiments it is evident that, during arousal and alertness, activation is produced in some intrinsic elements of the olfactory bulb by centrifugal influences from the mesencephalic tegmentum of the brain stem.

Centrifugal activation of the olfactory bulb during arousal may be a physiological mechanism for enhancing the sensitivity of the olfactory apparatus—so important for exploration of the environment in macroscopic animals such as the

cat. The observation that olfactory stimulation inhibits the "arousal discharges" in the olfactory bulb (13) supports this interpretation. Besides discharges from silent elements, cessation of background activity produced by the olfactory stimulus provides useful information to the brain.

The "arousal discharges" from the olfactory bulb may prove to be a useful index for objective recording of the degree of alertness in the cat.

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## Effects of Carnitine on Fatty-Acid Oxidation by Muscle

The function of carnitine ( $\beta$ -hydroxy,  $\gamma$ -betaine butyric acid) in vertebrate organisms is not known. Carnitine is widely distributed among microorganisms, plants, vertebrates, and invertebrates (1), but the richest source is muscle. Fraenkel and his colleagues (1) have shown that the meal worm *Tenebrio molitor* requires a growth factor, initially named vitamin B<sub>T</sub>, which has been identified as carnitine.

While examining the effects of muscle extracts on fatty-acid metabolism by liver, we had demonstrated that part of the action of muscle extracts could be simulated by the addition of carnitine, which resulted in augmented conversion of palmitate-1-C<sup>14</sup> to C<sup>14</sup>O<sub>2</sub> and ketones by both liver slices and homogenates (2, 3). Experiments have been performed in efforts to elucidate the site of carnitine action on the fatty-acid oxidase system. It has been established that carnitine effects cannot be duplicated by choline (3), and that carnitine cannot substitute for choline in preventing the development of fatty livers in rats maintained on a choline-deficient regimen; from these findings it was concluded that carnitine and choline influence different aspects of fatty-acid metabolism.

More recently, we have shown that the action of carnitine on the fatty-acid oxidase system is lost when the component enzymes from liver particulates are solubilized by treatment with deoxycholate or by homogenization of the particulates in distilled water in a Waring blender (4). Furthermore, addition of carnitine has no effect on the oxidation of palmityl-1-C<sup>14</sup>-coenzyme A (CoA) (4); this sug-

gests that carnitine action occurs prior to the formation of palmityl CoA in a particulate system.

Attempts to demonstrate an effect of carnitine on the long-chain fatty-acid activating enzyme described by Kornberg and Pricer (5) were negative, although a system was obtained which yielded proportionately increasing amounts of palmityl hydroxamate with the addition of rat-liver particulate enzyme preparations (4). The possibility remains that carnitine augmentation of long-chain fatty-acid oxidation is associated with facilitation of the transfer of palmitate to the active site of the fatty-acid oxidase particulate.

Results given in Table 1 indicate that the addition of carnitine enhances palmitate-1-C<sup>14</sup> oxidation by washed rat-skeletal-muscle particulates. The effect is less than twofold at zero albumin concentrations, but the percentage increase induced by carnitine is augmented as bovine serum albumin is added. Higher amounts of albumin depress fatty-acid oxidation, presumably by firmer binding of the palmitate (6, 7). In the presence of carnitine, the albumin-induced inhibition of palmitate oxidation is considerably lessened. The effects of varying the carnitine concentration are also shown in Table 1, where it can be seen that carnitine action occurs over a wide range of concentrations, including the reported carnitine concentrations in rat skeletal muscle (1).

Since carnitine effects are evident in the absence of added albumin, it is unlikely that carnitine exerts its action on the metabolism of palmitate exclusively by uncoupling palmitate-albumin binding sites. However, it is not impossible that palmitate-protein sites may be influenced by the presence of the dipolar carnitine molecule, which has both lipophilic and hydrophilic groupings. Other instances are known in which cationic detergents, for example, can disrupt lipid from lipoprotein complexes (8). At any rate, the addition of carnitine is followed by augmented palmitate oxidation by muscle particulates, and the relative magnitude of the effect is remarkably increased at higher albumin-to-palmitate ratios.

In addition to the results of the present study, indicating the inhibition by high albumin-to-palmitate ratios of palmitate oxidation by muscle particulates, previous work has shown that the albumin-to-palmitate ratio influences the amount of fatty acid oxidized by isolated rat hemidiaphragm segments (6). When the albumin-to-palmitate ratio exceeded 0.14, approximately, palmitate oxidation was depressed (6). This is a somewhat curious phenomenon, since the albumin-to-palmitate ratio in mammalian plasma is around unity if one assumes an albu-

min concentration of 4 g/100 ml, an albumin molecular weight of 69,000, and a plasma nonesterified fatty-acid concentration of 0.5 to 0.6 mM. Serum albumin has the property of strongly binding palmitate (7), and recent evidence indicates that blood nonesterified fatty acids are transported as an albumin-fatty-acid complex (9, 10).

There is no doubt that muscle readily captures nonesterified long-chain fatty acids (10) and that it can oxidize them to CO<sub>2</sub> under conditions of both rest and activity (6). Since the available data suggest that palmitate oxidation by muscle *in vivo* would be inhibited by an albumin-to-palmitate ratio of unity, it is relevant to ask by what means serum fatty acids are transported from blood vessels through the interstitial space to the active sites for fatty-acid oxidation in muscle cells. The present data suggest the possibility that carnitine in muscle in some manner serves the function of facilitating fatty-acid transfer to the fatty-acid oxidase sites (11).

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- Fed male Holtzman rats of approximately 200 g were killed by decapitation. The hind leg musculature, freed of gross fat and fascia, was rapidly removed and placed in ice-cold 0.14M KCl, buffered to pH 7.4 with 0.015M phosphates. After several washes the weighed muscle was homogenized in 5 vol of this solution in an ice-jacketed Monel blender for 10 sec at a time at 60-sec intervals, for a total of 60 sec. The homogenate was rehomogenized in a Ten Broeck apparatus, strained through one layer of cheesecloth, and centrifuged at 4°C in a Servall angle centrifuge at approximately 4400g for 15 min. The residue was resuspended in the same volume of KCl-PO<sub>4</sub> medium and recentrifuged, as described above. The washed particulates were resuspended in one-fourth of the original volume of KCl-PO<sub>4</sub> solution and rehomogenized in a Ten Broeck tissue-grinding apparatus; after this 1.0-ml aliquots were transferred to Warburg-type vessels to give a total final fluid volume of 2.5 ml, containing 280  $\mu$ mole of KCl; 6.5  $\mu$ mole of MgCl<sub>2</sub>; 30  $\mu$ mole of KH<sub>2</sub>PO<sub>4</sub> and K<sub>2</sub>HPO<sub>4</sub>, adjusted to pH 7.4; 5  $\mu$ mole of adenosine triphosphate as the sodium salt (Pabst); 2.9  $\mu$ mole of diphosphopyridine nucleotide (Sigma); 0.051  $\mu$ mole of cytochrome c (Sigma); 0.225  $\mu$ mole of CoA (Pabst); 0.265  $\mu$ mole of K-palmitate-1-C<sup>14</sup>, contain-

Table 1. The effects of carnitine on palmitate oxidation by muscle particulates (12).

Albu- min/ palmi- tate molar ratio	<i>dl</i> -Carni- tine hydro- chloride added ( $\mu$ mole)	Palmitate-1-C <sup>14</sup> converted to C <sup>14</sup> O <sub>2</sub> per 100 mg (dry weight) of muscle per 30 min ( $\mu$ mole)	
		Con- trol	Carni- tine
<i>Experiment No. 1</i>			
0	0.762	7.97	14.7
0.14	0.762	24.0	34.8
1	0.762	5.31	44.3
2	0.762	2.40	27.1
<i>Experiment No. 2</i>			
1	0.0254	7.79	9.30
1	0.254	7.79	25.2
1	2.54	7.79	61.5
1	10.2	7.79	54.8
1	19.0	7.79	20.5

ing 0.5  $\mu$ c of  $C^{14}$  (Nuclear) and crystallized bovine serum albumin as indicated (Armour). The *dl*-carnitine used was a gift from International Chemicals, Inc., Chicago, and was supplied as the hydrochloride. All flasks were incubated at 37°C for 30 min in air, after which 0.2 ml of 50-percent citric acid was added from a side arm to insure complete liberation of  $C^{14}O_2$  in the medium. The  $C^{14}O_2$  was trapped by alkali having filter paper immersed in the center well. The plating as  $BaCO_3$  and the calculations employed were the same as those previously reported (6). The dry weight of muscle aliquots used in each flask was approximately 25 mg. The experiments reported are representative of six experiments performed in a similar fashion.

2 September 1958

## Acrolein for the Control of Water Weeds and Disease-Carrying Water Snails

**Abstract.** Injection of the biocide acrolein into irrigation canals killed submersed weeds, thereby reducing flow resistance and increasing capacity. In a 20-mile canal, 150 gallons raised throughput from 311 to 552  $ft^3/sec$ . Acrolein also effectively eradicated aquatic snails; it promises to become a useful tool in the battle against *Schistosoma* blood flukes.

Irrigation is a blessing, for it turns deserts into productive lands, but there are certain drawbacks which must be overcome before the full benefits of irrigation can be realized. First, there is the menace of aquatic weeds, which, when left uncontrolled, soon render an irrigation system inoperative. Then, there is a menace to health, as several vectors of human and animal diseases are aquatic in habit. Various new irrigation systems in the tropics, for instance, introduce aquatic snails which are alternate hosts for a trematode worm (the blood fluke) which causes schistosomiasis (1).

Although ditch-bank weeds are difficult to control, submersed aquatic weeds constitute an even more difficult problem. It is the submersed weeds which cause the most trouble from the standpoint of reduced water flow in irrigation ditches (2). In the western United States alone farmers spend millions of dollars every year for the control of these weeds—commonly called “moss”—in canals and drains (3). Various mechanical methods—such as draining and drying, hand-cleaning, chaining, and dredging—for the control of submersed aquatic weeds in irrigation channels have been used extensively. In many situations, however, these methods have proved relatively inefficient or cumbersome, time-consuming, and expensive (4). Among chemical methods, use of copper sulfate was found to be effective on algae, but it was not generally effective on plants that were rooted in the bottom of the channel (4). Aromatic solvents such as crude xylenes gave effective control of submersed aquatic weeds but could be

used economically only in smaller channels (up to 50 to 70  $ft^3$  of water flow per second). Large amounts of solvent and emulsifier are required, and even when as much as 10 gal of solvent per cubic foot of water flow per second is applied in the channel, such an application is effective only for distances up to 5 mi without “booster” applications (4).

We have now developed a method, involving the use of a technical product the active ingredient of which is acrolein ( $CH_2=CH\cdot CHO$ ), which will control submersed aquatic weeds economically even in large canals (with water flow of 300  $ft^3/sec$  and over). Submersed weeds have been controlled as far as 15 to 20 mi below the point of application through the use of only 1 to 1.5 gal of acrolein per cubic foot of water flow per second over a period of 30 to 45 min. In less than 1 week after treatment the water-carrying capacity of such a large canal, 60 ft wide, nearly doubled, and the beneficial effect lasted for as long as 8 weeks before retreatment became necessary. Figure 1 shows the results of the water-weed control by acrolein in a large canal in Kern County, California (5). The major weed was the pond weed, *Potamogeton crispus*, but the chemical controlled all other submersed vegetation as well. After treatment the dead vegetation disintegrated and hence did not clog gates, weirs, and pumps, as happens after chaining (dragging). When deposited on the land, such masses of organic matter could be beneficial. Treated water, when used for irrigation, did not harm crops. Further studies, on possible acrolein residues in crops and on the toxicity of treated water with respect to farm animals, are being made.

Acrolein is a potent irritant and lachrymator, but in the hands of a skilled

operator with proper application equipment it can be applied safely and without irritation or discomfort.

Acrolein readily forms a true solution in water and travels down the canal as a chemical wave. The location of the acrolein wave in the canal can be detected easily with a drop of potassium permanganate solution in a test tube. There is no loss of the chemical due to breaking of emulsions, as there is with aromatic solvents.

Acrolein has been found highly effective against water snails in the canals treated. Against adults of the pond snail *Lymnaea bulimoides*, and also against the local planorboid snail, *Helisoma* (*Planorbis*) *trivolvis*, it was found to be twice as effective, in laboratory tests in  $\frac{1}{2}$ -gal jars, as sodium pentachlorophenate. The latter is commonly used for the control of bilharziasis (6). Against snail egg masses it was found to be more effective than copper sulfate, another agent commonly used as an aquatic molluscicide.

Acrolein is a potent sulfhydryl reagent and has been observed to destroy isolated enzyme systems. For instance, against urease it is nearly four times as toxic on a molar basis as ethyl maleimide, a common —SH reagent. Turgor in cells of the leaves of the water plant *Elodea densa* was maintained for a few hours after a dip in 1000 parts of acrolein per million, even though microscopic examination showed the cell interior to be destroyed. This observation leads to the conclusion that acrolein destroys —SH enzymes in the cytoplasm of the cell, unlike aromatic solvents, which act primarily on the plasma membrane.

Dosages required to kill *Elodea* leaf cells under laboratory conditions are small: 0.5 parts per million (ppm) for

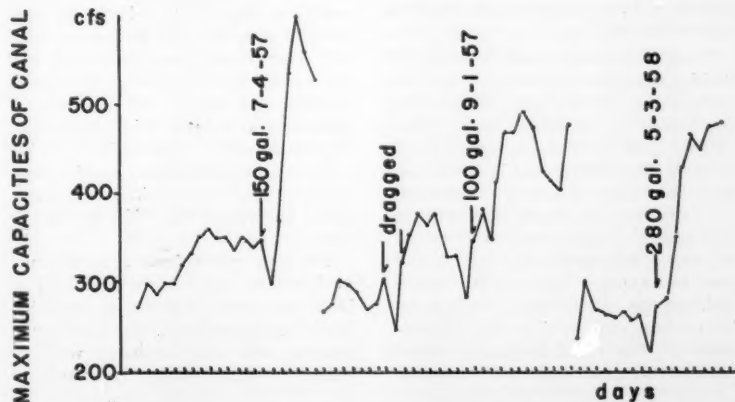


Fig. 1. Beneficial effect of acrolein on the water-carrying capacity of a large irrigation canal. The computed maximum capacity in cubic feet per second is plotted against time. The horizontal units are single days. The three trials reported refer to the same canal. The amount of acrolein and the date of treatment are indicated on each graph. The increase in flow is due to destruction of submersed water weeds by the chemical.



24 hours will destroy all cells. A 2-hour exposure to a dose of 5 ppm is equally effective. It has been found that the product of minimum concentration and minimum time of exposure is a constant. This constant is the effective dose; under laboratory conditions, this is of the order of 10 ppm-hours for *Elodea*. The dosage rule also holds under field conditions, but the dose applied is usually larger, for obvious reasons. There also appears to be a temperature factor. Preliminary tests indicate a  $Q_{10}$  of 2.

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2 September 1958

### A Substance in Liver Which Inhibits Thymine Biosynthesis by Bone Marrow

**Abstract.** A substance was extracted from rabbit liver which inhibited the incorporation of formate into bone-marrow thymine in vitro. In view of the important role of thymine biosynthesis in cell division, it is suggested that the inhibitor present in liver is a naturally occurring mitosis inhibitor.

Some of the factors which control cell division have been considered in a recent review (1). It was pointed out that duplication of deoxyribonucleic acid (DNA) must occur in the cycle of cell division. It is obvious that in certain tissues there must exist some mechanism for control of mitotic rate. The presence of a mitotic inhibitor was suggested by the recent finding that the injection of liver homogenates from adult animals inhibited the mitotic rate of regenerating rat liver (2). In view of the importance of DNA in cell division it seemed possible that there might exist in tissues

naturally occurring inhibitors of DNA biosynthesis which would then be mitosis inhibitors. This report presents experimental evidence for the occurrence of such an inhibitor (3).

The experimental procedure consisted of incubating rabbit bone-marrow homogenates or cell suspensions with sodium formate- $C^{14}$  and measuring the incorporation of the formate into ribonucleic acid (RNA) and DNA. The effect of various rabbit-liver preparations on the incorporation of the radioformate into nucleic acids was then determined. Bone marrow from normal rabbits was homogenized with 4 volumes of Robinson's medium (4), or a suspension of marrow cells was prepared with the same relative quantity of buffer. One milliliter of the marrow preparation was incubated with the liver fraction for 20 minutes in a Dubnoff incubator at 37°C, 8  $\mu$ C of sodium formate- $C^{14}$  was added, and the incubation was continued for 2½ hours. The final volume of incubation mixture was 4 ml. The incubation mixture was then fractionated by a combined Schneider, Schmidt-Thannhauser procedure (5), and the incorporation of the formate into RNA and DNA was measured.

In the first experiments it was found that under these conditions bone-marrow homogenates actively incorporated formate into DNA, whereas liver homogenates did not. It was further found that when liver homogenates were added to the bone-marrow homogenates there was marked inhibition of DNA biosynthesis.

Efforts were then made to concentrate the factor in liver which inhibited DNA biosynthesis by bone marrow. Rabbit liver was homogenized with Robinson's medium, heated to boiling, and filtered. The filtrate was batch-treated with Dowex-1 exchange resin. The filtrate was then lyophilized, and the dry powder was extracted with 95-percent ethanol. The ethanol was evaporated, and the powder was dissolved in Robinson's medium and assayed for inhibition of bone-marrow nucleic acid biosynthesis. Typical results are given in Table 1. Six milligrams of this alcohol-soluble material quite markedly inhibited the incorporation of formate into DNA by marrow homogenates.

The next experiments were designed to determine whether the inhibition in DNA biosynthesis was due to inhibition in thymine biosynthesis. One milliliter of marrow cells was incubated with 10  $\mu$ mole of deoxyuridine, 10  $\mu$ mole of

Table 1. Effect of liver fraction on the incorporation of formate- $C^{14}$  into nucleic acids by bone-marrow homogenates.

Addition	Counts	
	RNA	DNA
None	272	398
Liver fraction (6 mg)	177	78
Inhibition (%)	35	80

Table 2. Effect of liver fraction on the incorporation of formate- $C^{14}$  into thymine by bone-marrow cells.

Addition	Thymine counts
None	5105
Liver fraction (6 mg)	366
Inhibition (%)	92

adenosinetriphosphate, and 50  $\mu$ mole of sodium succinate. Other details of incubation were as previously described. At the termination of the incubation the reaction mixture was made 2N with perchloric acid and heated on a steam bath for 30 minutes. This treatment extracted DNA and hydrolyzed all thymine nucleotides (free thymine nucleotides and DNA thymidylic acid) to free thymine. After neutralization of the perchloric acid with KOH, carrier thymine was added and acetol osazone (6) formed and was counted. The results are given in Table 2. It may be seen that the liver fraction strongly inhibited the biosynthesis of thymine by bone marrow.

These experiments demonstrate the presence of a substance in liver which inhibits thymine biosynthesis by bone-marrow cells. In view of the requirement for thymine biosynthesis in cell division it appears that this substance may be a naturally occurring mitosis inhibitor.

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12 September 1958

## Meetings

### Biological Sciences

The ninth annual meeting of biological societies under the auspices of the American Institute of Biological Sciences was held at Indiana University, Bloomington, 24-28 August 1958, with a record attendance of 4800. The 50th anniversary celebration of the American Phytopathological Society, bringing nearly 1000 plant pathologists to Bloomington, did much to swell the attendance. There were 1487 individual and symposium papers during the four days of meetings by the members of 27 societies. Among these many papers, the ones outlined below represent a few of the more interesting or significant.

W. T. McDonough (University of Maryland) reported on the significantly higher yields obtained from "bearded" Duram wheat in semiarid regions in the West. In the eastern portion of the country, beards have little or no effect on yield. Through the use of radioactive carbon (in  $\text{CO}_2$ ) it was found that 41 percent of all carbohydrate manufactured by the head was made by the beards. At low soil-moisture percentages, the beards remained functional as suppliers for the developing kernels when other plant parts were contributing little or nothing.

W. R. Breneman and M. Carmack (Indiana University) have demonstrated that spray-dried preparations of cold-water extracts of *Lithospermum rudersale* ("lithosperm") inhibit gonadotropins in the chick. Inhibition of pregnant mare serum and anterior pituitary luteinizing hormone occurred after in vitro mixture with lithosperm for 1 hour at room temperature. Injection of lithosperm into cockerels for 10 days prevented testis growth and androgen secretion, indicating the inhibition of endogenous gonadotropin. Endogenous thyrotropin inhibition was also noted. The spray-dried powders used in the study retained activity for 2 years. The preparations have also been demonstrated to inhibit egg-laying in hens, to inactive glucagon, and both to inhibit the uptake of  $\text{I}^{131}$  by thyroid glands and to suppress the release of  $\text{I}^{131}$ .

A hint about the mechanisms of variation in animal growth was supplied by P. J. Van Alten (Michigan State Uni-

versity), who grafted tissue from chickens onto chick embryo membranes. Use of adult duodenum tissue significantly increased the size and weight of the embryo's heart, liver, and spleen, while it decreased the over-all weight of the embryo. Apparently a direct transfer of antigen occurred from the grafted tissue to the embryo organ. In certain types of grafts, however, there was some evidence that this antigenic action had an effect on the whole embryo, rather than on the corresponding organ alone.

In man and other mammals, sectioning of the optic nerve results in irreparable loss of vision. In fishes and amphibians, however, the optic nerve is able to regenerate after complete transection. H. L. Arora and R. W. Sperry (California Institute of Technology) reported on the complete recovery of color vision in cichlid fish following optic-nerve section. All normal training evidence indicates that fish discriminate between such colors as blue, red, green, yellow, and orange, responding to differences in wavelength rather than merely to variations in intensity. The finding that the ability to discriminate color was reinstated in its original form following section and regeneration of the optic nerve implied that the reconnection of optic axon in the brain takes place in an orderly manner and is governed by fiber specificities associated with color perception as well as by the topical specificities associated with direction. The fact that color-discrimination training survived suggests that the memory engrams, whatever their nature, must be located more centrally than the optic axon synapses.

W. R. Tulecke (Chas. Pfizer & Co.) told of inducing *Ginkgo* pollen to form many-celled tissues in culture. Most of the pollen grains germinated in a normal fashion, but many proceeded to form definite tissue masses. Both arginine and urea were used successfully to support this growth.

"Homing" in bats has been demonstrated by H. C. Mueller (University of Wisconsin), and the theory has been offered that it is accomplished by means of their built-in echolocation system. Mueller believes that the bats, released some distance from "home" in unfamiliar territory, "sound-picture" the ter-

rain until it becomes familiar, then rapidly head for home territory.

Red dye and radioactive chemicals, introduced into freshly cut pine stumps, were found a day or two later in the sapwood of nearby living trees. F. H. Bormann and B. F. Graham (Dartmouth College) reported. Of 84 stumps tested, 41 were found to be grafted to at least one neighboring tree. In a number of cases the neighboring trees were in turn grafted to others. Considered until recently merely a botanical curiosity, natural root grafts promise to be of growing interest to those concerned with plant diseases, forest management, and water supplies. Organisms causing Dutch elm disease, oak wilt, and so on may move from tree to tree through such grafts; tree poisons, used by foresters, could move from the treated trees to nearby untreated ones.

Two new drugs, glycarbylamide and nicrabazin, are proving more effective against cecal coccidiosis of poultry than any of four other compounds currently popular (nitrofurazone, Bifuran, sulfaquinoxaline, and Trithiadol). The six chemicals were compared by D. K. McLoughlin and D. K. Chester (U.S. Department of Agriculture, Beltsville). All infected groups of chicks treated with medicants showed less mortality, less evidence of severe hemorrhage, and better weight gains than did infected but untreated groups. Results with the compounds tested differed only in the degree of protection given the treated birds.

Cesium-137, a poisonous fission product from power reactors and atomic fallout, with long radiation life, is concentrated by many aquatic organisms. *Rhizoclonium* concentrated cesium at a level 1530 times the amount in its environment. *Euglena* took out 69 percent of the cesium in 11 days. *Chlorella*, after having been killed with formaldehyde, was able to concentrate cesium at a level 418 times the environmental level in 8 days. Even when high concentrations of potassium were present in the water, many of these organisms seemed to "prefer" the cesium. These findings were reported by L. C. Williams (Furman University).

B. Greenberg (University of Illinois College of Pharmacy) suggests that the common housefly may not be such a disease-carrying villain after all, for it sheds bacteria as it progresses from egg to adult. As the egg hatches into a maggot, tens of millions of bacteria accumulate in the digestive tract. This count drops about 90 percent as the maggot develops a pupal casing. Most of the remaining bacteria are left behind in the pupal case, and the fly emerges relatively "pure," with an average count of 500 internal bacteria. Even these are not contained in the digestive tract, where they could be spread in waste products. With an external count of approximately

500 bacteria, the newborn fly is in a relatively clean condition.

W. S. Bailey and W. E. Ribelin (Auburn School of Veterinary Medicine) told parasitologists that, although cancer occurs commonly in dogs, it is rare in the esophagus. When it does occur there, the esophageal worm is "an inciting cause." It is of interest that all dogs examined with cases of esophageal cancer were hounds or bird dogs.

Hobart M. Smith (University of Illinois) warned that diagnosis of poisonous snakebite should not rest solely upon the existence of swelling. Bites that go deep into muscle usually produce great swelling, while bites injecting venom just under the skin can be just as dangerous without producing any swelling. Test of blood-clotting time is a more reliable basis for diagnosis. Smith pointed out, as a sad example, the recent death of Karl P. Schmidt of the Chicago Natural History Museum from the bite of a South African boomslang snake; there had been no great swelling or pain, but the bite was fatal.

Species of *Calvatia gigantea*, the giant puffball, have been found to have tumor-inhibiting properties with respect to mice, according to a team from Michigan State University and the Sloan-Kettering Institute—E. H. Lucas, R. Byerum, J. A. Stevens, C. C. Stock, D. A. Clarke, and H. C. Reilly. Only one type of mouse cancer has been tested to date, and the scientists emphasize that the specific responsible substances are not yet known.

A fungus that apparently facilitates the healing of human burns was discussed by J. E. Peterson (University of Missouri). The fungi appeared in the wounds of a severely burned boy as a white, cottony growth. It had apparently been introduced when the boy's father rolled him directly on the earth to put out the flames. Doctors were able to do clean-up surgery in half the usual time and believed that the growth was responsible for the rapidity of healing. Upon isolation in pure culture, the fungus was identified as *Fusarium roseum*.

Interest in Mexican hallucinogenic mushrooms (teonanacatl) has been revived in recent years, doubtless due to increased research in mental illness. In 1957 an expedition visited the Mazatec country to collect and identify mycological material. Rolf Singer, R. Ames, and S. I. Stein (Chicago) reported on their findings. The following species have now been established as producing psychoneurophysiologic effects: (i) *Psilocybe cubensis*, (ii) *P. caerulea*, and (iii) *P. mexicana*, and possibly *P. candidipes*. A later trip added *P. muliercula* and *P. aztecorum* to the list. All six species have the following characteristics in common: The fruiting bodies, when scratched, handled, or dried out, stain

blue; the odor and taste are distinctive—the odor, that of fresh flour or cucumbers; the taste, more or less astringent or unpleasant. The type of vegetation in which psilocybes occur is not uniform; the habitat varies from tropical and temperate montane to truly tropical rain forest and to subalpine frigid. Stein reported personally on the dramatic effects of eating *P. cubensis* and discussed reports on other species. The recent isolation of a substance, Psilocybin, from *P. mexicana* makes possible the standardization of dosage and indicates the possibility of further isolation of active substances.

G. C. Decker (Illinois Natural History Survey) termed the large-scale use of insecticides "one of the important technological developments of the 20th century." Decker said emphatically that "the general sum and substance of all research in this field [insecticides] has failed to indicate any significant public health hazards," and he continued, "It is not surprising that there have been numerous instances where wildlife of varied types have been adversely affected by insecticides. At the same time preponderance of evidence indicates such incidents can in practically all cases be traced to carelessness, to accidents, or to instances of outright experimentation."

To assist in meeting the current need to provide high-school and college teachers with up-dating courses, the American Society of Zoologists, the Society of Protozoologists, and the National Association of Biology Teachers jointly sponsored an eminently successful refresher course in "Advances in Protozoology" (J. O. Corliss, *Science*, this issue). Organized by T. M. Sonneborn, it consisted of three sessions of lectures, one of discussion, one of demonstrations, and two of motion pictures. The response to this course definitely indicated the value of such cooperative ventures and the need for similar efforts at succeeding meetings.

#### Plant Pathology

The American Phytopathological Society, in celebration of its 50th anniversary, conducted a four-day international symposium which attracted many of the world's outstanding scientists in the field. Robley C. Williams (University of California) told plant pathologists, "At the present time, in the investigation of plant virus structure, the electron microscope has its greatest potential usefulness in examination of the form taken by the substructure of partially disintegrated viruses. What is needed is improvement in chemical techniques whereby many viruses can be degraded in a controlled manner. The form of the building blocks of the 'spherical' viruses is a particularly intriguing matter owing to the evidence

of the geometrical nicety with which they are filled into the complete polyhedral structure. It is my prediction that this specific area of research is the one in which electron microscopy of the future will have its greatest triumphs."

In discussing the landmarks in the use of chemicals to control plant diseases, George L. McNew (Boyce Thompson Institute) stated, "The unresolved problems continue to loom large on the horizon. The farmers of the U.S. still endure a loss of over 7 percent of crop productivity because of disease. This is a toll of \$3 billion annually that may be alleviated by new types of chemicals and better methods of using those that are already available. There is little room for complacency or self satisfaction from the discovery of new chemicals to date." McNew continued, "The principles of how to design molecules for specific attributes are rapidly being established. With all the versatile possibilities of organic chemistry to draw upon, the plant physiologist can look forward with confidence to resolving the problems still before him."

E. C. Stakman (University of Minnesota), who delivered the principal address of the anniversary celebrations, traced the role of plant pathology in the scientific and social development of the world. Quoting from van Loon, that "The history of man is the record of a hungry creature in search of food," Stakman showed that man "had to become at least a practical biologist in order to develop a civilization; and he has had to become a continually better biologist in order to preserve it." The first written records of plant diseases occurred in Hebrew and Egyptian literature; these were followed by very concise records in the writings of Greek and Roman naturalists, poets, and historians. In a scholarly fashion, Stakman traced the complete and steady development of the science of plant disease through the ages to our modern state of knowledge. The speaker concluded, "Plant pathology has helped to satisfy man's hunger for food and his thirst for knowledge. Is not that reason for pride?"

ILEEN E. STEWART  
*American Institute of Biological Sciences, Washington, D.C.*

#### Refresher Course in Protozoology

At the recent meetings (24 to 28 Aug. 1958) of the American Institute of Biological Sciences, held on the campus of Indiana University, Bloomington, a major attraction was a "refresher course" entitled "Advances in Protozoology," sponsored jointly by the American Society of Zoologists, the Society of Protozoologists, and the National Association of Biology Teachers, and supported by



the National Science Foundation. This was not the first refresher course to be held at AIBS meetings, but it was surely the most extensive and intensive one and was unusual in several respects.

#### Aims and Organization

The principal aims of the course, which offered sessions during the better part of two full days and evenings, were to update the level of high-school and college teaching in areas involving protozoa and to excite renewed interest in these microscopic organisms by reviewing recent progress in the major fields of research in protozoology. The high degree of interest shown both by the formal participants and the large audiences in attendance at the several sessions reflected in large part the infectious enthusiasm of the project's energetic organizer, T. M. Sonneborn (Indiana University).

The several unique features of the course included the availability of a mimeographed "Syllabus," which contained 58 pages of comprehensive information (in annotated outline form, with helpful bibliographies) relative to the nine major lectures; an unusually extensive demonstration session, to which mimeographed guides were available, involving nearly 100 exhibits; a special discussion session on the problems and philosophy of "teaching protozoa" at all educational levels; and several showings of a diversified collection of protozoological films.

The nine lectures, arranged with a minimum of overlap and with ample discussion periods, were followed by the demonstration session. Movies were organized into several shows which ran concurrently in different locations and were repeated over a three-day period.

#### Lecture Sessions

J. O. Corliss (University of Illinois) opened the first lecture session, speaking on the broad subjects of systematics and evolution and reviewing some general facts concerning the numbers, diversity, and ubiquity of protozoa. Their complex nature defies a simple definition, and many problems are involved in attempting to establish natural systems of classification. Under the topic of evolution Corliss included consideration of the two major hypotheses of origin of the eumetazoa from protozoa (the colonial-integration notion versus the compartmentalization-of-a-single-cell theory) as well as ideas concerning various interrelationships among the protozoa themselves, at both highest and lowest taxonomic levels. Dorothy Pitelka (University of California, Berkeley), in reviewing advances in protozoan cytology, emphasized particularly the ultrastructural studies now possible with the electron microscope. Of special value to many in the audience

was her clear exposition of the basic homologies which exist between structures possessed by single protozoa and those of single metazoan cells, such as mitochondria, Golgi apparatus, cilia, microsomes, and the endoplasmic reticulum.

P. B. Weisz (Brown University) brought the first session to a close with a lucid lecture on morphogenesis in protozoa. Particularly important was his well-documented presentation of the roles of the nuclear apparatus and of the cytosome in such processes as regeneration; his points were illustrated mainly by references to experimental work carried out on the large heterotrichous ciliate *Stentor*. He concluded with emphasis on the fact that certain basic nucleocytoplasmic interrelationships play an essential part in all morphogenetic phenomena carefully studied to date.

R. W. Siegel (University of California, Los Angeles) led off the second lecture session with a discussion of protozoan life cycles in which sexuality is known or suspected to play a significant role. In particular, he included some of the many problems concerning life histories of the foraminiferidans, the higher zooflagellates, and the ciliates, treating them from a comparative point of view. The classical question of immortality of unicellular organisms was discussed in the light of modern researches; Siegel concluded that asexual species show potential immortality in their life cycle, but that many sexual species seem to obey the "rule of Maupus" in passing through a series of natural stages of immaturity, maturity, senescence, and death. G. H. Beale (Institute of Animal Genetics, Edinburgh) discussed the kinds of genetic problems that can be approached with protozoan material in general, mentioning the species so used to date and reviewing the subject of Mendelian inheritance in protozoa. The antigen system of *Paramecium aurelia* was discussed in some detail, with particular consideration of the three classes of determinants involved: nuclear genes, cytoplasmic states, and factors of the external environment.

W. Balamuth (University of California, Berkeley) spoke on the subject of controlled cultivation of protozoa, treating, among other topics, terminology, technology, general applicability, specific patterns of nutritional requirements in different groups of protozoa, and recent advances in experimental studies of growth as related to controlled cultivation. Perhaps one of the most exciting advances in population-growth work is the recent success in achieving a synchronized fission cycle in mass cultures of the ciliate *Tetrahymena*. S. H. Hutner (Haskins Laboratories), in the last address of the second lecture session, emphasized in still greater detail how protozoa are becoming valuable tools in biochemical research, especially for studying metabolic

systems in higher animals. Primarily as a result of the advent of chemically defined culture media, permitting precise environmental control, a number of species are now being widely used in biochemical work, some in such important areas as cancer research. Forms covered in Hutner's discussion included members of these ten important genera: *Euglena*, *Ochromonas*, *Peranema*, *Trypanosoma*, *Crithidia*, *Leishmania*, *Tetrahymena*, *Paramecium*, *Stylonychia*, and *Podophyra*.

E. R. Becker (Iowa State College and, currently, Arizona State College) reviewed the present status of our knowledge concerning major groups of symbiotic protozoa which exhibit classical host-parasite relationships. His talk included discussion of the microfauna of the digestive tract of termites, the closely related higher zooflagellates of the wood-eating roach *Cryptocerus*, and the ciliate fauna of the stomach of ruminants. In the syllabus, the life cycle of the all-important malarial organisms also was considered. Topics discussed by Becker within the framework of the subjects mentioned above ranged from experimental defaunation of the host to evolutionary trends in the phylogeny of the symbiont, from the complex physiological interrelationships of the host and its microfauna to the special relationship of the sexual cycle of the flagellates in *Cryptocerus* to the roach's own life cycle.

C. P. Read (Johns Hopkins University) followed Becker with an appropriate discussion of the comparative biochemistry of parasitic protozoa—a talk dedicated to the memory of Asa C. Chandler, long a leader in American parasitology, news of whose untimely death in Europe had just reached Bloomington. Read was concerned with four major aspects of his broad subject: catabolism of parasitic protozoa; anabolism of parasitic protozoa; specific patterns of metabolism in the host-parasite relationship; and applied comparative biochemistry of parasitic protozoa. Perhaps most revealing to the large audience, composed mainly of general biologists, was his presentation of the great advance being made in our knowledge of drug resistance, chemotherapy, and pertinent pharmacology by virtue of laboratory studies carried out on protozoa as experimental animals.

It should be mentioned that a number of the lecturers included cross references to personal demonstration material. Corliss posted nine large charts depicting "phylogenetic trees" dating from the 1880's to unpublished work kindly contributed by specialists for the different protozoan groups; most of these were also used in his talk, as illustration of both the complexity and the diversity of classificational and evolutionary schemes. Dorothy Pitelka joined half a dozen other

top-notch electron microscopists interested in protozoa in a beautiful display of electron micrographs on the walls of one of the corridors in Jordan Hall, and Weisz demonstrated microsurgical techniques employed in regeneration and transplantation work on *Stentor*. Siegel presented clear-cut charts representing a general scheme of protozoan life cycles, with separate illustrations of foraminiferan and ciliate life cycles. Hutner demonstrated production of apochlorotic strains of *Euglena* with streptomycin, and uses of certain protozoa as microbiological assay organisms. Balamuth exhibited the greatest quantity of supporting material of all the speakers in his extensive and painstaking demonstration of cultures of a dozen different species of protozoa and of techniques for maintaining such cultures for both research and teaching purposes. Many demonstrations by others, discussed below, also served to illustrate and complement the lecture material.

#### Discussion Session

A special discussion session was devoted to methods, problems, and philosophy of teaching, particularly as related to presentation of protozoan material. L. E. Noland (University of Wisconsin) ably acted as a participating chairman; A. M. Elliott (University of Michigan) presented considerations at the college level; P. R. Fordyce (Broad Ripple High School, Indianapolis, and, currently, Oak Park High School, Oak Park, Ill.) discussed aspects at the high-school level. Fordyce's remarks in particular created discussion from the audience, since he was the only person on the entire refresher-course staff directly concerned with students at the pre-college level.

From the scheduled talks and the spontaneous comments from the floor, it was evident that protozoa serve a most useful purpose in biology courses but that they might be used even more extensively if methods of collection, maintenance, and demonstration were more generally known.

#### Motion Pictures

The motion-picture sessions were extended over one afternoon and three evenings. R. Vishniac's (New York City) *The Not Too Simple Protozoa* surely was the most picturesque, having been filmed in color by means of his own special technique. Other movies of particular value as teaching aids covered many topics, ranging from life and death of a cell, conjugation in *Paramecium bursaria*, general biology of *Chaos chaos* (or *Pelomyxa*), excystment in *Colpoda*, feeding mechanisms in *Bresslaia*, and movement in *Labyrinthula* to nuclear transplantation in *Amoeba* and exocytic

throcytic stages of avian malaria in chick embryos and tissue culture.

Two outstanding sets of foreign films, which reached Bloomington too late for inclusion in the printed program, also were shown. The first comprised an assorted group of films portraying features in the general biology of a good number of protozoa representative of all four major groups. These were produced by K. C. Wingstrand and A. Øye (Copenhagen). The second set represented an organized microcinematographic study of food-getting in a score of species of ciliated protozoa, the feeding methods shown ranging from those exhibited by the insatiable herbivorous gymnostomes to the highly specialized adaptations revealed in the habits of the carnivorous suctorians. J. Dragesco (Paris) was primarily responsible for these excellent films, taken over a period of ten years in the laboratories of E. Fauré-Fremiet (Collège de France).

A number of the films shown are available for purchase or rental; many would certainly serve as excellent teaching aids. Inquiries concerning costs, addresses, dates of delivery, and so on, of the two groups of foreign films may be directed to T. M. Sonneborn.

#### Demonstration Session

Probably the most unusual aspect of the refresher course was the demonstration session. Over 80 exhibits, occupying some 13 separate locations in Jordan Hall of Biology, represented nearly every phase of protozoology. The exhibitions not only covered all kinds of protozoa, plus methods and techniques of studying and using them, but also represented the major centers of protozoological work, at both teaching and research levels, in the United States; nearly 100 contributors were directly involved in the planning and execution of this very impressive affair. Of indispensable aid were two mimeographed "Guides to the Demonstrations," one more detailed than the other, which listed all exhibits, with locations, and also served as a fine summary of the entire demonstration session.

Lack of space precludes mention here of all the demonstrations and demonstrators, but some idea may be given of their scope and variety through a brief enumeration of the major areas or subjects covered. Parasitic protozoa were well represented by exhibits, including extensive ones, of protozoan and host materials associated with malaria, coccidiosis, "blackhead" in birds, and the symbiotic relationships of protozoa with termites, wood-roaches, and ruminants. Cytological techniques, apparatus (and even results!) were in evidence in many of the rooms; one entire corridor was devoted to displays of electron micrographs of protoplasmic constituents of the

protozoan body, representing the work of half a dozen American laboratories. Culture collections [including R. C. Starr's (Indiana University) collection of algae], culture techniques, and even some of the elaborate methods involved in modern approaches to nutritional, biochemical, and radiobiological studies were available for examination; here, as in the case of certain cytological techniques, the spectator was sometimes invited to "do-it-yourself," to his delight and profit.

Extensive demonstrations of ecology, life cycles, sexuality, evolution, phylogeny, and systematics (by means of living material as well as charts, drawings, and photomicrographs) filled one large room, including most of its wall space. Every aspect of the complex life of *Paramecium aurelia* (all syngens), and related species, was portrayed—principally, but not exclusively, by members of the Sonneborn school of paramecium biology. Such demonstrations covered not only culture methods, syngen distribution, mating reactions, serotypes, general and specific cytology and genetics (including "kappa" particles), and ultrastructure but also precise growth requirements, electromigration, paper electrophoresis, chromatography, and radioautography. W. J. van Wagtenonk deserves special mention in connection with the last-mentioned topics, and Ruth Dippell merits commendation for organizing so competently much of the local material involving *Paramecium*.

A large number of the demonstrations, as mentioned above, were quite appropriately slanted towards applicability in teaching about the protozoa. This was particularly true of a variety of cytological, experimental, and cultural techniques, such as those displayed by groups from the following schools or laboratories: Indiana University, University of California (Berkeley, Los Angeles, and Davis campuses), State University of Iowa, University of Illinois, Howard University, University of Michigan, University of Wisconsin, Argonne National Laboratory, and Haskins Laboratories.

Originality was apparent on all sides; in this connection a few specific demonstrations may be mentioned: the huge three-dimensional scale models (in plaster) of rhizopod amoebae by E. C. Bovee (University of Florida); the striking demonstration (for which mice were used as the experimental animals) of the strength of the dinoflagellate toxin responsible for mussel poisoning on the West Coast, by L. Provasoli and J. J. A. McLaughlin (Haskins Laboratories); the magnificent illustration of the phylogenetically important *Gonium-Volvox* sequence, in which living material was used exclusively, by R. C. Starr (Indiana University); use of the "Tartar Cyclo-



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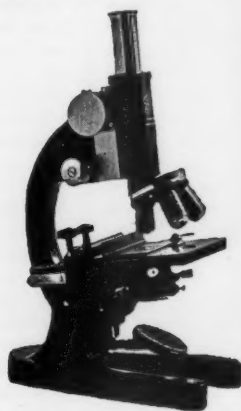
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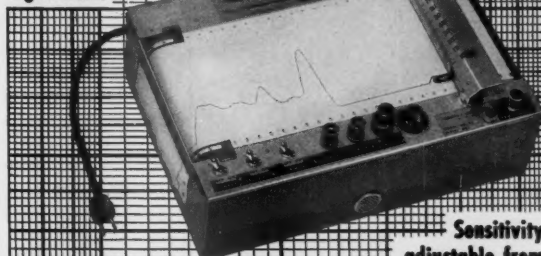
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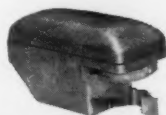
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paramecetome," the easily constructed whirling razor-blade device so convenient for slicing up small ciliates to determine their powers of regeneration, by E. W. McArdle (University of Illinois); the living exhibition of the "love-dance" entered upon by sexually reactive ciliates of the hypotrichous genus *Stylonychia* immediately prior to conjugation, by L. E. Downs (Sierra College), and the elaborate portrayal of the cytology and complex life cycles of heterotrichous and vorticellid ciliates, by H. E. Finley and his colleagues.

One of the most educational "one-man" shows was the demonstration by L. E. Noland (University of Wisconsin) of the dynamic ecological interrelationships existing in biotic communities of the microscopic world. Surrounded by a host of microscopes, aquaria, and smaller culture chambers, and very busy helping others while enjoying himself thoroughly, Noland exemplified the true protozoologist who makes no apology for loving the protozoa purely for their own sake.

JOHN O. CORLISS

Department of Zoology,  
University of Illinois, Urbana

## Fluid and Solid Mechanics

The Midwest Conference on Fluid and Solid Mechanics will be held at the University of Texas, Austin, 9-11 September. This meeting, which will be the sixth Conference on Fluid Mechanics and the fourth Conference on Solid Mechanics, forms another element in a series of these conferences. The conferences have taken place at approximate intervals of 2 years, previous ones having been held at the University of Illinois, Ohio State University, the University of Minnesota, Purdue University, and the University of Michigan.

Although the conferences are organized on a regional basis, there are no restrictions on attendance or on the residence of authors of technical papers. A General Conference Committee for handling initial arrangements consists of Walter L. Moore, professor of civil engineering; Enrico G. Volterra, professor of engineering mechanics; and Milton J. Thompson, professor of aeronautical engineering. Abstracts and titles of proposed papers should be submitted to Thompson, who is chairman, before 1 April. Complete manuscripts will be required by 1 June, so that the proceedings may be distributed at the opening of the conference. Inquiries concerning other details of the conference should also be addressed to Thompson.

The University of Texas, as a part of its support of the conference, has arranged some invited lectures on important aspects of mechanics. Samuel A.

Schaaf of the University of California will present a lecture on "Recent Progress in Rarefied Gas Dynamics Research," while Lloyd H. Donnell of Illinois Institute of Technology will talk on "New Developments in Shell Theory." At a dinner meeting, Sydney Goldstein of Harvard University will speak on "Recent Progress in Applied Mechanics."

Cosponsors of the Midwestern Conference include some ten national scientific societies, the Office of Scientific Research of the Air Force, the Office of Naval Research, the Office of Ordnance Research of the Army, and the National Science Foundation.

## Wildlife Federation

One of the highlights of the 23rd annual convention of the National Wildlife Federation, to be held at the Sheraton-McAlpin Hotel in New York, 27 February-1 March, will be a thorough discussion of abatement of water-pollution. Ernest Swift, the federation's executive director, has announced that a six-member panel will discuss pollution, beginning at 2 P.M. on 28 February. The participants will include Rep. John A. Blatnik (Minn.), author of the Federal Pollution Control Act of 1956, which established the grants program for construction of sewage treatment plants.

A panel discussion touching on all phases of insect and plant-pest chemical controls and their effects, particularly upon wildlife, will highlight the opening-day program.

The convention will be attended by delegates representing 50 affiliated wildlife federations and conservation leagues in the states and the District of Columbia. Immediately following the federation convention is the 23rd annual North American Wildlife Conference, sponsored by the Wildlife Management Institute. It will be held in the Statler-Hilton Hotel, New York, 2-4 March.

## Geochemical Society

The annual meeting of the Geochemical Society was held at St. Louis, 6-8 November, under the auspices of the Geological Society of America. Forty-eight papers on geochemistry were presented, of which 14 were chiefly concerned with phase relations, 11 with distribution of trace and minor elements, 11 with geochronology, and 12 with general geochemistry. Eight additional papers were read by title. The retiring president of the Geochemical Society, Farrington Daniels, gave an address on "Kinetics and Thermoluminescence in Geochemistry."

Newly elected officers for 1958-1959

are J. F. Schairer, president; T. F. W. Barth, vice-president; George T. Faust, treasurer; E. W. Heinrich, editor; K. B. Krauskopf, secretary.

## Radar

More than 500 scientists from the United States, Great Britain, and Canada met 27-30 January at the University of Michigan in Ann Arbor to explore new ways of utilizing radar for scientific and military purposes. The conference was sponsored jointly by the Army, Navy, and Air Force. It was conducted by the Radar Laboratory of the Willow Run Laboratories under the auspices of Project Michigan, a combat-surveillance research and development program being conducted by the university for the U.S. Army Combat Surveillance Agency.

This, the university's fifth Annual Radar Symposium, was the largest gathering of radar specialists held at the university to date, according to J. A. Boyd, director of the Willow Run Laboratories. The participants discussed a wide range of military and civilian radar developments, from new methods of detecting battlefield targets to the utilization of radar to enable space ships to avoid sinking into the dustlike cover of the moon when they land on it. Research into supersonic flight radar, space communication, and ICBM guidance systems received special emphasis. The sessions were classified and therefore not open to the general public.

## Forthcoming Events

### March

3. National Vitamin Foundation, 14th annual, New York, N.Y. (W. Rubin, Director of Public Information, National Vitamin Foundation, Inc., 149 E. 78 St., New York, N.Y.)

6. Molecular Structure and Vital Processes, symp., New York, N.Y. (N. Weiner, American Chemical Soc., 84-40 101 St., Richmond Hill, N. Y.)

6-7. Recent Developments in Microscopy as Applied to Biology, symp., New York, N.Y. (C. Tufts, Sylvania Research Laboratories, Bayside, N.Y.)

7. American Chemical Soc., Oklahoma Div., tetrasectional meeting, Tulsa. (J. W. Conant, ACS, Grand River Chemical Div. of Deere and Co., Pryor, Okla.)

8-9. American Broncho-Esophagological Assoc., Hot Springs, Va. (F. J. Putney, 1712 Locust St., Philadelphia, Pa.)

8-9. American Laryngological Assoc., Hot Springs, Va. (J. H. Maxwell, University Hospital, Ann Arbor, Mich.)

8-11. American Soc. of Photogrammetry, 25th annual, Washington, D.C. (J. H. Wickham, Jr., 1959 ASP-ACSM Consecutive Meetings, 610 Montgomery St., Alexandria, Va.)

8-12. Aviation Conf., Los Angeles,



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Calif. (O. B. Schier, ASME, 29 W. 39 St., New York, N.Y.)

9-11. International Acetylene Assoc., annual, New Orleans, La. (IAA, 30 W. 42 St., New York 17.)

10-12. American Laryngological, Rhinological and Otolological Soc., Hot Springs, Va. (C. S. Nash, 708 Medical Arts Bldg., Rochester 7, N.Y.)

11-14. American Cong. on Surveying and Mapping, Washington, D.C. (J. H. Wickham, Jr., 1959 ASP-ACSM Consecutive Meetings, 610 Montgomery St., Alexandria, Va.)

13-14. American Otolological Soc., Hot Springs, Va. (L. R. Boies, University Hospital, Minneapolis 14, Minn.)

13-15. Alabama Acad. of Sciences, Auburn, (H. M. Kaylor, Dept. of Physics, Birmingham-Southern College, Birmingham, Ala.)

14-15. Southwestern Soc. of Nuclear Medicine, 4th annual, New Orleans, La. (S. B. Nadler, SSNM, 1520 Louisiana Ave., New Orleans 15, La.)

15-20. American College of Allergists, San Francisco, Calif. (M. C. Harris, 450 Sutter St., San Francisco.)

16-19. American Assoc. of Petroleum Geologists, Soc. of Economic Paleontologists and Mineralogists, 44th annual, Dallas, Tex. (W. A. Waldschmidt, AAPG, 311 Leggett Building, Midland, Tex.)

16-20. American Inst. of Chemical Engineers, Atlantic City, N.J. (F. J. Van Antwerpen, AIChE, 25 W. 45 St., New York 36.)

16-20. National Assoc. of Corrosion Engineers, 15th annual conf., Chicago, Ill. (NACE, Southern Standard Bldg., Houston, Tex.)

16-20. Western Metal Exposition and Cong., 11th, Los Angeles, Calif. (R. T. Bayless, 7301 Euclid Ave., Cleveland 3, Ohio.)

17-19. National Health Council, Chicago, Ill. (P. E. Ryan, 1790 Broadway, New York, 19.)

17-20. Organization of Research, 4th intern. symp., Milan, Italy. (I. Svergie, Produktivitetsnamnden, Linnegatan 87, Stockholm O, Sweden.)

18-25. International Social Science Council, 4th general assembly (by invitation), Paris, France. (C. Levi-Strauss, Secretary-General, International Social Science Council 19, avenue Kleber, Paris.)

19-21. Society for Research in Child Development, NIH, Bethesda, Md. (Miss N. Bayley, Laboratory of Psychology, National Inst. of Mental Health, Bethesda 14, Md.)

19-22. International Assoc. for Dental Research, 37th general, San Francisco, Calif. (D. Y. Burrill, Northwestern Univ., 311 E. Chicago Ave., Chicago 11, Ill.)

20. New Jersey Acad. of Science, annual, New Brunswick. (H. L. Silverman, 361 Highland Ave., Newark 4, N.J.)

23-24. Theory of Fluid Flow through Porous Media, 2nd conf., Norman, Okla. (C. G. Dodd, School of Petroleum Engineering, Univ. of Oklahoma, Norman.)

23-26. Institute of Radio Engineers, natl. conv., New York, N.Y. (G. L. Haller, IRE, 1 E. 79 St., New York 21.)

24-27. American Meteorological Soc., general, Chicago, Ill. (K. C. Spengler, AMS, 3 Joy Street, Boston, Mass.)

27-28. Michigan Acad. of Sciences, East Lansing. (D. A. Rings, Univ. of Michigan, Dept. of Engineering, Ann Arbor.)

27-28. Pennsylvania Acad. of Sciences, Gettysburg. (K. Dearolf, Public Museum and Art Gallery, Reading, Pa.)

28. South Carolina Acad. of Sciences, Columbia. (H. W. Freeman, Dept. of Biology, Winthrop College, Rock Hill, S.C.)

29-3. Latin American Congress of Chemistry, 7th, Mexico D.F., Mexico. (R. I. Frisbie, Calle Ciprés No. 176, Zone 4, Mexico, D.F.)

30-31. Third Teratology Conf., Portland, Ore. (D. L. Gunberg, Dept. of Anatomy, Univ. of Oregon Medical School, Portland.)

30-1. American Orthopsychiatric Assoc., San Francisco, Calif. (M. F. Langer, 1790 Broadway, New York 19.)

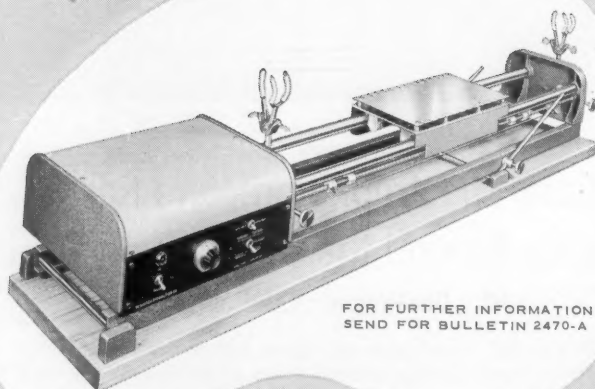
30-12. Bahamas Medical Conf., 7th, Nassau. (B. L. Frank, 1290 Pine Ave., W. Montreal, Canada.)

31-2. American Power Conf., 21st annual, Chicago, Ill. (N. S. Hibshman, AIEE, 33 W. 39 St., New York 18.)

31-2. Symposium on Millimeter Waves, 9th, New York, N.Y. (H. J. Carlin, Microwave Research Inst., 55 Johnson St., Brooklyn 1, N.Y.)

(See issue of 16 January for comprehensive list)

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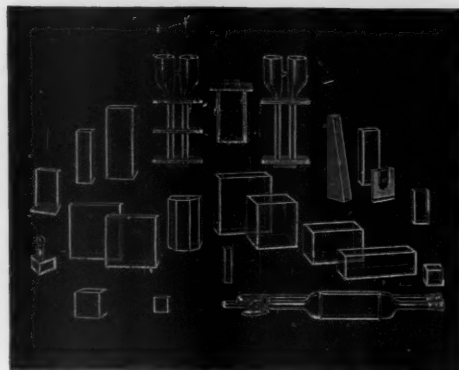
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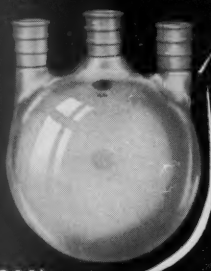


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## Equipment

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Science does not assume responsibility for the accuracy of the information. A coupon for use in making inquiries concerning the items listed appears on page 350.

■ **TWO-CHANNEL RECORDER** is an integral unit, including amplifiers, on a single chassis. Frequency range is from d-c to beyond 100 cy/sec. Measuring range is 10 mv to 400 v. Input impedance is 10 megohm. Direct ink writing is used. Chart speeds range from 1 to 125 mm/sec. (Brush Instruments, Dept. 595)

■ **PRESSURE TRANSDUCER** converts pressures in the range of 0 to 1000 lb/in.<sup>2</sup> into frequency of its electrical output. Frequency is inversely proportional to applied pressure. Conversion to frequency is provided by a vibrating wire stretched between an anchor point and a pressure-sensitive metal diaphragm. The wire is set into motion in a permanent magnetic field by an alternating current through the wire so that frequency is controlled by the resonant frequency of the vibrating wire. (B. J. Electronics, Dept. 616)

■ **CONTROLLED-ATMOSPHERE ENCLOSURE** is available in transparent plastic or in a combination of steel and plastic. The enclosure is circular in design and has a transparent hemispherical dome. Evacuation can be accomplished by connection to a vacuum pump, or oxygen can be removed by an accessory gas purification system. Round construction permits more than one person to assist in operations being performed in the box. Diameter is 24 in. (Controlled Atmosphere Enclosure Manufacturing Co., Dept. 617)

■ **RECORDING MIRROR CAMERA** records position-time relationships for events in the hypervelocity range. The camera consists of a cast-aluminum main housing, a rotating mirror, an f/2.5 lens, two curved film holders, and an air-turbine drive. The hexagonal mirror rotates at 3000 rev/min, providing writing speed of 4 mm/μsec for a total writing time of 50 μsec. Mirror speed can be reduced. The camera provides a continuous record, the rotating mirror sweeping the image onto curved film strips. Focus remains fixed for a given series of exposures. (Avco, Dept. 620)

■ **FREQUENCY STANDARD** is a transistorized crystal oscillator with oven control for the range 400 to 2000 cy/sec. The instrument can withstand shock up to 100 g or vibration from 0 to 2000 cy/sec at 10 g. Frequency stability is ±0.002 percent over a temperature range -55° to +85°C. Output is a square wave. (Dynamics Corp. of America, Dept. 622)

■ **SPECTRUM ANALYZER** covers frequencies from 10 to 44,000 Mcy/sec with one tuning head. Resolution is continuously variable from 1 to 80 kcy/sec. Sweep width is continuously adjustable up to 70 Mcy/sec to make possible either wide-band observation of the over-all range or high-resolution analysis of small segments of the range. Amplitude scales are calibrated for voltage and power. (Panoramic Radio Products Inc., Dept. 614)

■ **RESISTANCE BRIDGE** covers the range 1000 to  $1.1 \times 10^{14}$  ohm. Accuracy is better than ±0.2 percent up to  $10^{13}$  ohm. Test potential is continuously variable to 1000 v. A set of nine plug-in laboratory-standard resistors and a battery power supply are furnished. (Mid-Eastern Electronics Inc., Dept. 630)

■ **TRANSISTOR CIRCUIT SYNTHESIZER** provides four transistor stage panels and a master metering panel. Common-base, common-emitter, and common-collector circuit configurations can be assembled. A floating battery supply powers the assembly. Transistors of p-n-p and n-p-n types can be combined. A curve tracer, incorporated in the metering panel for use with an external oscilloscope, allows visual display of the plot of collector voltage versus collector current. The value of the base current bias can be measured concurrently with the visual display. (National Electronics Laboratory, Inc., Dept. 623)

■ **GAS ANALYZER** for traces of toxic gases and vapors in air or process streams operates by electrical measurement of the decrease in conductivity of samples caused by removal of ions by the contaminant being measured. Continuous ionization is produced by radioactive material in the measuring ionization chamber. Two chambers are series arms in a Wheatstone bridge. A difference in conductivity is observed when a sample constituent reacts with a reagent in the detector cell to form a particulate dispersion that traps the ions. (Mine Safety Appliance Co., Dept. 625)

■ **TEMPERATURE TRANSDUCERS** for liquid, air, and surface temperature measurement feature 200-msec response. The temperature-sensitive element is a deposited platinum film. Element resistance is 100 ohm, and resistance/temperature coefficient is 0.0018/°C. Accuracy of the liquid type is ±0.5 percent or 0.1°F in the liquid-oxygen range. Accuracy of the air and surface types is ±0.5 percent from -100° to +500°F and ±1 percent for the -100° to +1200°F range. (Arnoux Corporation, Dept. 628)

JOSHUA STERN  
National Bureau of Standards

(Continued from page 298)

Similar remarks are apposite to Bateson's provocative and revealing paper. He, too, complains that I have failed to fully quarry the anthropological mine, and sets out to unearth some riches that I neglected. What he discovers are some anthropological—or, shall we say, Batesonian—exports to biology, psychology and epistemology. Such exports fail to impress me, but that is comparatively minor. The difficulty with Bateson's letter is that it can be taken by an unfriendly critic as evidence for the thesis that the status of anthropology does not differ from that of sailing or mining. For it is well known that problems of, and discoveries by, miners and sailors have resulted in advances of physics. Whether Bateson would take this as denigration of anthropology or elevation of mining is unknown to me. I do know that his letter was motivated by the conviction that anthropologists have discovered many interesting and important truths about men and their cultural activities. I share Bateson's convictions. Nevertheless, I doubt whether anthropological findings have thus far led to comprehensive theories. And my doubts on that score were not allayed by Bateson's otherwise instructive paper.

Hansen impresses me as a zealous knight who is prepared to break many a lance for the discipline to which he has sworn fealty. Unfortunately, he either has never taken, or has forgotten, his vows of chivalry, and thus, alone among my critics, Hansen finds nothing in my paper that he can bless. Perhaps this judgment is too strong. Except for the unfounded suggestion that I criticize learning theory for failing to supply us with teleological explanations, he neglects to comment upon my views about recent anthropological employment of learning theory. His silence on the score may indicate consent. In point of fact, his reticence is puzzling, but not as much as some of his explicit inferences.

Thus, I do not know how Hansen concluded that I wanted anthropologists to restrict themselves to historical statements. It is well known that explanations in history are given without the aid of any special class of historical theories, and my hint was that similar conditions may some day obtain in anthropology. Incidentally, Hansen more or less agrees with this point, when he notes that anthropologists are "doing their utmost to reveal the wonders of social phenomena," and that they will attempt to do so even if it means borrowing theories from other disciplines. But this means the autonomy of anthropology is not a necessary condition for its success. That, among other things, was the point of the analogy between anthropology and history, an analogy that aroused the ire of my critic and led him to forget parts of my paper. Had I wanted to give an air of preten-



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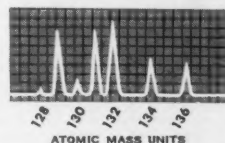
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tious novelty to my remarks, I would never have quoted Maitland.

Hansen insists that functionalists are well aware of my criticisms, and therefore seems to be disputing not the truth, but the novelty of my remarks. I will bypass the problem of originality, and simply take Hansen's letter as a datum against his own view. For he fails to understand, much less agree with, my critical observations. Thus, I was not concerned with the danger of teleology, but with the dispensability of certain locutions, and with the conditions that must be satisfied by any explanation, teleological or otherwise. It was the failure of functionalists to produce bonafide explanations, not their employment of a terminology, that perturbed me.

I was equally disturbed about the empirical status of some statements spon-

sored by anthropologists. I reported my findings, and to controvert them, Hansen administers a methodical spanking. To answer my charge that some functionalistic propositions are banal, he observes that most scientific theories are based on banal foundations. "In fact," he continues, "one indication of the soundness of a scientific theory is the simplicity of its basis." I surmise that even Hansen, in his more charitable moments, will find a difference between banality and simplicity, and will discover little that is banal about the foundations of, let us say, quantum electrodynamics. At that time, I am sure he will also find it difficult to specify a sense for the phrase "foundation of a theory" and to find a method for measuring simplicity.

To meet my claim that some statements are false, and others tautologous,

Hansen points to the self-corrective nature of scientific inquiry. False statements can be replaced by more warranted ones, and tautologies, when incorporated within theories, some of whose premises are true, but not logically true, can lead to fruitful empirical results. But the possibility of self-correction is no proof that the correction called for is not in order. Or perhaps Hansen merely set out to show that the presence of faulty theories is no proof of the demise of a discipline. But I can assure him that I never set out to bury anthropology. I only questioned some currently employed theories, and for that Hansen and others seem ready to banish me as a nonscientific infidel.

SIDNEY MORGENBESSER

Department of Philosophy,  
Columbia University, New York

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Applications should be addressed to Dr. D. E. Smith, Dean, Faculty of Arts and Science, University of Alberta, Edmonton. They should include a complete curriculum vitae, transcripts of university records, a photograph, and the names of at least three persons who have agreed to send references. Applications will be received until 1 April 1959. 1/23

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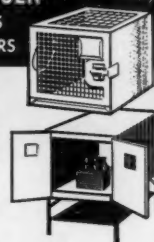
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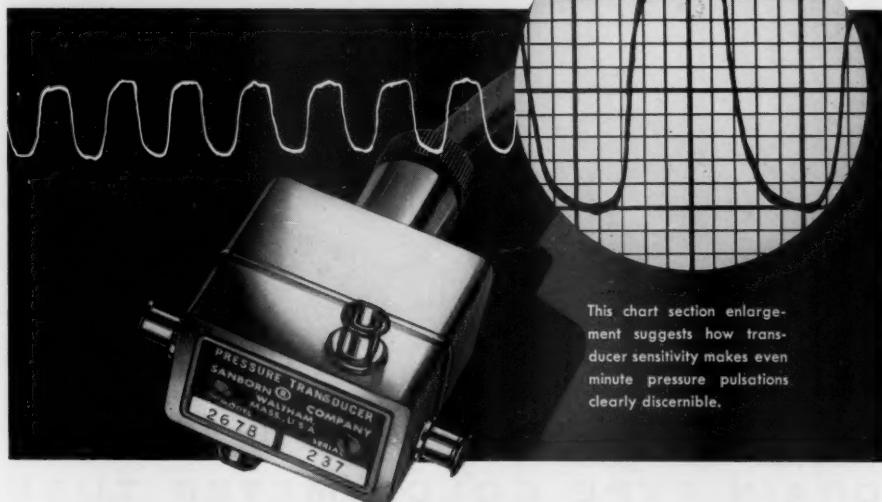
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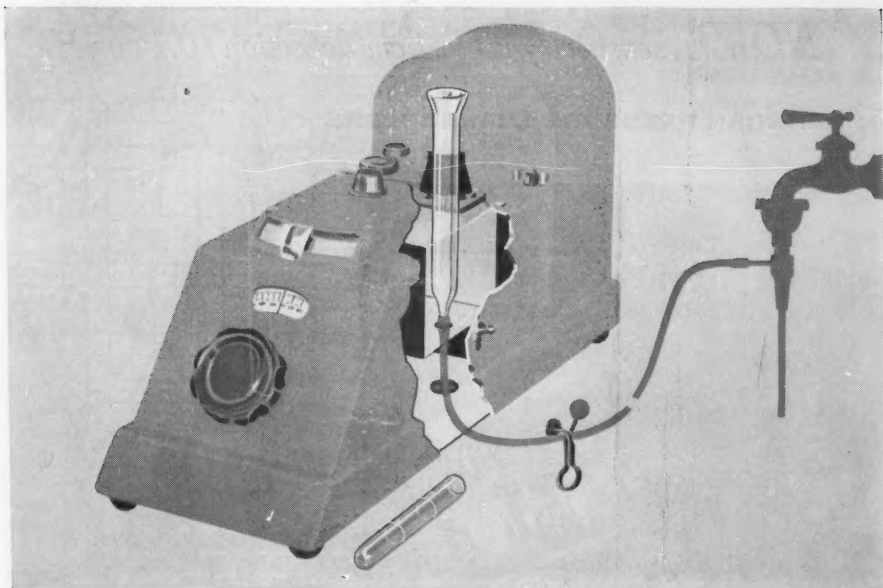
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